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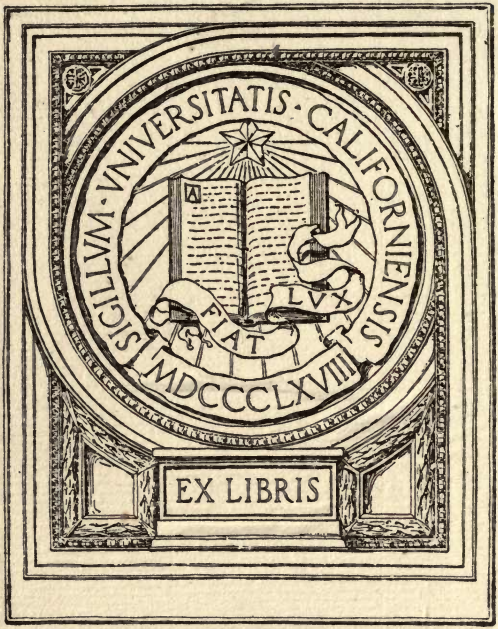
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THE THEORY

OF THE

GAS ENGINE.

By DUGALD CLERK.

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PREFACE.

The continued existence of the Gas Engine as a competitor with the Steam and Hot Air Engines is no longer an open question. It has fairly passed that phase of the experimental stage which determines its fitness to aid in the solution of the problem of the economical conversion of heat into mechanical work.

The improvements of the last few years have brought this motor into a conspicuous position.

We believe this essay of Mr. Clerk's to be the best presentation of the theory of tke Gas Engine that has yet appeared.

EDITOR OF MAGAZINE.

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The Theory of the Gas Engine.

The practical problem of the conversion of heat into mechanical work has long occupied the minds of engineers and scientists; the steam engine is a partial solution, but although perfect as a machine, its efficiency is so low that it can hardly be considered as satisfactory and final. As the result of the best modern practice it may be taken that the steam engine does not convert more than 10 per cent. of the heat used by it into work, and this in engines of considerable size and with boilers and furnaces fairly efficient. In small engines it is much less, indeed it is certain that few among the thousands of steam engines in daily use below 6 HP. give an efficiency greater than 4 per cent. The great cause of loss is the amount of heat necessary to change the water from the liquid to the gaseous

state, most of this heat being rejected with the exhaust either into the condenser or the atmosphere. Many attempts have been made to use liquids of lower specific heat than water, and requiring less heat for evaporation, the principal being alcohol, ether and carbon bisulphide, but for obvious reasons no success has been attained.

To heated air as a means of obtaining power, the objection of loss by latent heat does not apply, the air is already in the gaseous state, and any heat added at constant volume increases the temperature, and therefore the pressure, without the complication of change of physical state. A high efficiency would therefore be expected, and according to Professor Rankine the efficiency of the fluid in the engines of the "Ericsson" was about 0.26 ; the efficiency of the furnace was however low, and accordingly the actual efficiency of the engine was no higher than that of the best steam engines now in use. In the "Stirling" hot-air engine, he found the efficiency of the fluid to be

0.3 with a higher efficiency of furnace than in Ericsson's.

In the Ericsson engine the air was heated at constant pressure, the volume augmenting and the power being given by the increased volume of the air as it entered the motor cylinder from the reservoir into which it had been compressed. The mean effective pressure was only 2.12 lbs. on the square inch; the size and friction of the engine for a given power was enormous. In the Stirling engine the air was heated at constant volume with increase in pressure, the power being obtained by subsequent expansion; the mean available pressure was 37 lbs. per square inch, and the friction of the engine only amounted to one-tenth of the total indicated power. Both engines used the now well known contrivance, the regenerator, which was the invention of Dr. Stirling, and which is the cause in both of the high efficiency.

The failure of these engines was due to the rapid burning out of the cylinder bottoms by the direct action of the fire,

it being found impossible to heat the air rapidly enough to the required temperature without maintaining the temperature of the metal surfaces much higher than the maximum temperature to be attained by the air. To overcome this slow heating of the air when in mass has been the object of many inventors, and a type has often been proposed with a closed furnace, and the air forced through this furnace keeping up the combustion, the hot products going to the motor cylinder and there doing work. This method of internal heating, however, introduces difficulties as grave as exist in the external method. The hot gases having to pass through pipes and valves to the motor cylinder renders it impossible to maintain a very high temperature without damage to the machine. Sir George Cayley was the first to make and work experimentally an engine of this type.

In view of these futile attempts, until very recently hot air was considered as among the failures of the past, and it was believed that, imperfect as the steam

engine is, nothing was likely to succeed in producing a better result.

The great progress made in recent years with the gas engine, and its advance from the state of an interesting but troublesome toy to a practical powerful rival of the steam engine, has shown that air may after all be the chief motive power of the future. In the gas engine chemical considerations greatly modify the theory and prevent it from ranking as a simple hot-air engine; but to be thoroughly understood it is better first to consider the power to be obtained from air under certain theoretical conditions.

Three well defined types of engines have been proposed—

(1.) An engine drawing into its cylinder gas and air at atmospheric pressure for a portion of its stroke, cutting off communication with the outer atmosphere, and immediately igniting the mixture, the piston being pushed forward by the pressure of the ignited gases during the remainder of its stroke. The

in-stroke then discharges the products of combustion.

(2.) An engine in which a mixture of gas and air is drawn into a pump, and is discharged by the return stroke into a reservoir in a state of compression. From the reservoir the mixture enters into a cylinder, being ignited as it enters, without rise in pressure, but simply increased in volume, and following the piston as it moves forward, the return stroke discharges the products of combustion.

(3.) An engine in which a mixture of gas and air is compressed or introduced under compression into a cylinder, or space at the end of a cylinder, and then ignited while the volume remains constant and the pressure rises. Under this pressure the piston moves forward, and the return stroke discharges the exhaust.

Several minor types have been proposed and many modifications of these three methods are used. A thorough understanding of these, however, renders

it possible to judge the merits of any other.

Types 1 and 3 are explosion engines, the volume of the mixture remaining constant while the pressure increases. Type 2 is a gradual combustion engine in which the pressure is constant but the volume increases.

The author, in the course of his experiments on gas engines, has found that $1_{3}537°$ Centigrade is the temperature usually attained by the ignited gases in his engine, and he has accordingly investigated the behaviour of air under different conditions at this temperature.

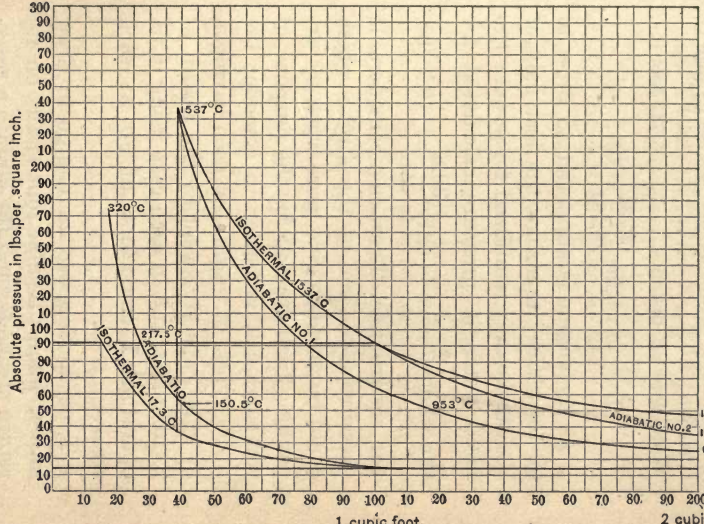
Type 1. Suppose an engine to have a piston with an area of 144 square inches and a stroke of 2 feet. Let the piston move through the first half of its stroke drawing into the cylinder air; let enough heat be immediately added to this air to cause it to rise instantly to 1,537° Centigrade, and the piston continue moving forward under the pressure produced. If there be no loss of heat through the sides of the cold cylinder, but the temperature

of the air fall only through performing work, how much work would be done when the piston completes its out-stroke?

The air before the heat is added is supposed to be at a temperature of 17° Centigrade (about 60° Fahrenheit), and the ordinary atmospheric pressure. In Fig. 1 the line marked adiabatic No. 2 is the curve showing the work which would be obtained under the supposed conditions. Fig. 2. is the indicator diagram such an engine would furnish. It is not necessary here to detail the calculations. With this paper is given a table of the data used, so that the numbers may be verified. The following are the results:

1 cubic foot of air (at 170° Centigrade, and 760 millimetres mercury) remaining at constant volume requires to heat it to 1,537° Centigrade, an amount of heat equivalent to.....	26,762 foot-lbs.
Maximum pressure in lbs. per square inch above atmosphere.....	76.6 lbs.
Pressure at the end of stroke per square inch above atmosphere....	19.6 lbs.

Fig.1.



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Mean pressure during available part of stroke..... } 39.8 lbs.

Temperature of air at the end of stroke.. ..... } 1,089° C.

Work done on piston.....5,731 foot-lbs.

$$\text{Duty of engine } \frac{5,731}{26,762} = 0.21.$$

As the engine is supposed to draw in air for half of its stroke, the last half of the stroke only is utilized for power; the mean available pressure calculated for the whole stroke is only $\frac{39.8}{2} = 19.9$ lbs. per square inch. There is a considerable pressure at the end of the stroke which could be made to give more work by expanding further; but for the purpose of comparison it is better to consider the three types of engine as each having a cylinder capacity swept by the piston of 2 cubic feet, and in each case using in its operation 1 cubic foot of air at each stroke.

Type 2. Suppose an engine to draw into a pump 1 cubic foot of air, on its return stroke forcing the air into a reservoir at a pressure of 76.6 lbs. per square inch above the atmosphere. The motor

Pressure upon atmosphere in lbs.per square inch.
250
200
150
100
50
0
0 1 2 3 4 5 6 7 8 9 10 1 2 3 4 5 6 7 8 9 10
1 cubic foot
2 cubic feet
1537°C
17°C
1089°C Atmo-sphere line

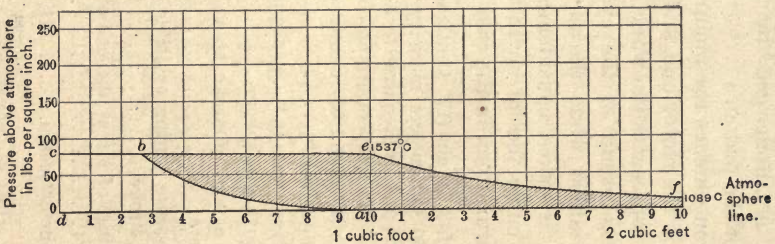
Fig.2

piston is now at the beginning of its out-stroke, and as it moves forward air from the reservoir enters the cylinder, but as it enters it is heated to 1,537° Centigrade, without rise in pressure; the motor piston sweeps through 2 cubic feet.

Fig. 3 shows the indicated card of this engine. *a b c d* is the pump diagram. Air at 17° Centigrade is taken in, compressed without loss of heat, the temperature rising under the compression to 217°.5 Centigrade. When it is equal to the pressure in the reservoir it is forced into the reservoir, as is shown on the line *b c*.

In all the operations no loss or gain of heat is assumed, except in doing work or in work being done on the air. In the motor diagram from C to E the air is flowing from the reservoir following the piston, and the temperature is 1,537° Centigrade during the whole admission. At *e* the communication with the reservoir is cut off, and the temperature falls while the air is expanding doing work, until it reaches the end of the stroke, when the

Fig. 3


Pressure above atmosphere
In lbs. per square inch.
250
200
150
100
50
0
b
c
e 1537° C
f
1089° C
Atmo-
sphere
line.
d 1 2 3 4 5 6 7 8 9 a 10 1 2 3 4 5 6 7 8 9 10
1 cubic foot
2 cubic feet

exhaust is discharged by the return stroke of the piston.

For convenience the pump diagram is shown on the motor one, and the shaded portion represents the work done by the air as the result of the cycle. As the heat is added while the air expands in volume, it takes considerably more to raise a cubic foot of air to the required temperature than in the case of type 1.

1 cubic foot of air (17° Centigrade and 760 millimeters mercury) at constant pressure requires to heat it from the temperature of compression 217°.5 to 1,537° Centigrade heat equivalent to.....	32,723 foot-lbs.
Maximum pressure in lbs. per square inch above atmosphere.....	76.6 lbs.
Pressure at end of stroke above atmosphere.....	19.6 lbs.
Mean pressure during available part of stroke.....	47.1 lbs. per square inch.
Temperature of air at the end of stroke.....	1,089° Centigrade
Work done on piston.....	11,759 foot-lbs.

$$\text{Duty of engine } \frac{11,759}{32,723} = 0.36.$$

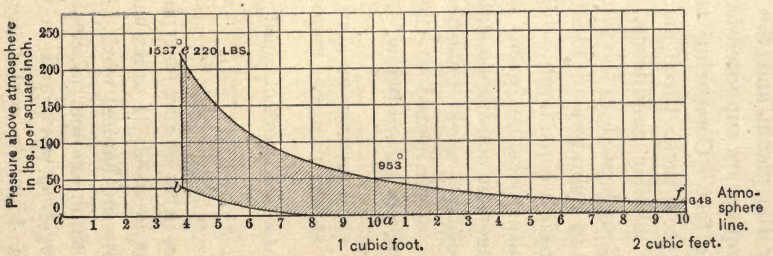
Type 3. Suppose an engine to draw into a pump 1 cubic foot of air, on its return stroke forcing it into a reservoir at a pressure of 40 lbs. above the atmosphere. The motor piston is now at the beginning of its out-stroke, and as it moves forward air from the reservoir enters the cylinder while the piston sweeps through 0.39 cubic feet. At this point communication is cut off, and the temperature suddenly raised to 1,537° Centigrade. Hitherto the air has remained at the temperature of compression 150°.5. The pressure goes straight up to 220 lbs. above the atmosphere. This is shown at Fig. 1, and also at Fig. 4, which is the diagram of this type of engine. *a b c d* is the compression diagram; *a b e f* the motor diagram. The piston continues to move forward, and the air expands doing work. At the end of the stroke the pressure has fallen to 8.4 lbs. per square inch above the atmosphere.

1 cubic foot of sir (17° Centigrade, and 760 millimeters mercury) at constant volume requires to heat it from the temperature of compression 150°.5 Centigrade to 1,537° Centigrade heat, equivalent to....	24,416 foot-lbs.
Maximum pressure in lbs. per square inch above atmosphere.....	220 lbs.
Pressure at end of stroke.....	8.4 lbs.
Mean pressure during available part of stroke.....	47.8 lbs. per square inch.
Temperature at middle of stroke	953° Centigrade.
Temperature at end of stroke..648° Centigrade.	
Work done on the piston.....11,090 foot-lbs.	

$$\text{Duty of engine } \frac{11,090}{24,416} = 0.45.$$

Fig. 5 shows the most important modification of this type; in it, instead of a separate reservoir, a space is left at the end of the cylinder, into which the piston does not enter, and in this space is compressed the gases forming the inflammable mixture. The rise in pressure therefore commences at the beginning of the stroke instead of when the piston has traveled out. In this diagram the volume

Fig.4


Pressure above atmosphere in lbs. per square inch.
250
200
150
100
50
0
c
d
1537° e 220 LBS.
953°
b
f
648
Atmosphere line.
1 2 3 4 5 6 7 8 9 10 a 1 2 3 4 5 6 7 8 9 10
1 cubic foot.
2 cubic feet.

swept by the piston and the clearance space together are supposed to be equal to 2 cubic feet. Comparing the results obtained from these three modes under precisely similar conditions, the same weight of air heated to the same degree, and used in cylinders of identical capacity, there is a considerable difference in the results possible even under the purely theoretic conditions stated.

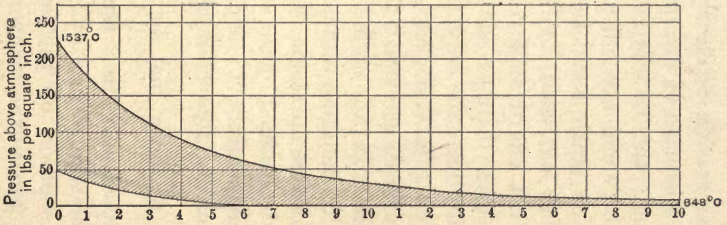
The relative work obtained from 1 cubic foot of air heated to the assumed temperature is shown below.

RESULTS FROM ENGINES OF EQUAL VOLUME SWEPT BY MOTOR PISTON.

Type				
1.	5,731 foot-lbs.	work obtained	0.21	duty.
2.	11,759 "	" "	0.36	"
3.	11,090 "	" "	0.45	"

That is, in an engine of type 1, if 100 heat-units be used, 21 units will be converted into mechanical work. In type 2, with the same amount of heat, 36 units will be given as work, and in type 3 no less than 45 units would be converted into work.

Fig.5


Pressure above atmosphere in lbs. per square inch.
250
200
150
100
50
0
1537°C
648°C
0 1 2 3 4 5 6 7 8 9 10 1 2 3 4 5 6 7 8 9 10
PARTS OF THE STROKE.

The great advantage of compression over no compression is clearly seen, by the simple operation of compressing before heating; the last type of engine gives for the same expenditure of heat 2.1 times as much work as the first. Compression, as used by the second type, does not afford so favorable a result; but even then the advantage is apparent, 1.6 times the effect being produced. By a greater degree of compression before heating even better results are possible. In an engine of type 3 expanding to the same volume after ignition as before compression, the possible duty D is determined by the atmospheric absolute temperature T′, and the absolute temperature after compression T; it is $D=\frac{T-T'}{T}$ whatever may be the maximum temperature after ignition. Increasing the temperature of ignition increases the power of the engine, but does not cause the conversion of a greater proportion of heat into work. With any given maximum temperature

the smaller the difference between that temperature and the temperature of compression, the greater is the proportion of added heat converted into work with any given amount of expansion. The greater the compression before ignition, the more closely the two temperatures come together, and the higher is the duty of the engine ; neglecting in the meantime the practical conditions of loss What compression does is to enable a great fall of temperature to be obtained due to work done with but a small movement of the piston. In type 1 when the piston has reached the end of its stroke, the increase from the moment of ignition is only from one volume to two volumes, while in type 3 with the same total volume swept by the piston, it increases from one volume to five volumes. In the one case the ratio of expansion is two, while in the other it is five. This will be readily seen in Figs. 2 and 4. Now this increased expansion is not obtained at the cost of loss average pressure ; in type 1 the mean available press-

ure over the whole stroke is nearly 20 lbs. per square inch, while in type 3 it is 38.5 lbs. per square inch; that is, the compression engine for equal size and piston speed has nearly twice the power of the other.

In the compression engine with a maximum temperature of 1,537° Centigrade, the final temperature is 648° Centigrade, while in the other, with the same maximum temperature, the final temperature is 1,089° Centigrade. It is true that by expanding sufficiently the same final temperature can be obtained without compression, but the average pressure will be low, and consequently less available for the production of power. To produce anything like an expansion of five times without compression the pressure would fall below the atmosphere, and it would be necessary to expand into a partial vacuum, and use a condenser and vacuum pump, as is done in the steam engine. Compression makes it possible to obtain from heated air a great amount of work with but a small

movement of piston, the smaller volume giving greater pressures, and thus rendering the power developed more mechanically available. The higher the maximum temperature the greater the amount of compression which can be used advantageously. There is a degree of compression for every temperature, beyond which any increase causes a diminution of the power of the engine for a given size.

The compression in the author's engine is 40 lbs. per square inch above the atmosphere, and he has accordingly confined himself to the comparison of engines employing this amount of compression with those using no compression. Now, seeing that this difference is produced between engines of types 1 and 3 by the simple difference of cycle, when there is no loss of heat through the sides of the cylinder, the question arises which engine would give the greatest effect, which engine in actual practice, with a cylinder kept cold by water, would come nearest to theory? In which

of the engines would there be the smaller loss of heat?

The amount of heat lost by a gas in contact with its enclosing cold surfaces depends, first, on the difference in temperature between the gas and the cooling surfaces; secondly, on the extent of surface exposed; and, thirdly, on the time of exposure. It would be very difficult to make an accurate numercial comparison between the engines, but all to be shown is, that in the one the loss of heat must be less than in the other.

To compare the two engines, take equal movements of the pistons from a maximum temperature of 1,537° Centigradê. In the engine working without compression this temperature is attained at the middle of its stroke, when the piston has moved through 1 cubic foot; the average temperature, while it moves to the end of its stroke, is about 1,300° Centigrade.

Now, in the compression engine the maximum temperature is attained at a point when the piston has moved through

0.39 cubic foot: suppose it to move to 1.39 cubic foot, it has moved through 1 foot in the same time as the first engine. Then, as the temperature at the middle of the stroke is 953° (Fig. 4) it follows that the average during this movement is not higher than 1,000° Centigrade, but the space containing the heated air has increased from 0.39 cubic foot to 1.39 cubic foot, and with it the cooling surface; whereas the space containing heated air in the first engine has, during the same amount of movement, increased from 1 cubic foot to 2 cubic feet. It follows that as the temperature in the compression engine is 1,000° Centigrade during the same time as the temperature in the first engine is 1,300° Centigrade, and as the surface in it for cooling is also less, the amount of heat lost by the air must be less in the portion of the stroke under consideration. During the portion of the stroke remaining, 0.61 cubic foot, the temperature of the heated air is low, falling to 648° Centigrade at the end of the stroke; it follows that

very small comparative loss results. Altogether the loss of heat by the compression engine will be the least.

It will be seen from Fig. 1 that there is a further cause of advantage. While the pressure and temperature are falling on adiabatic line 1, the work done by 1 cubic foot of air on expanding to the middle of the stroke at a temperature of 953° Centigrade is 7,888 foot-pounds, from 953° Centigrade to 648° is 3,202 foot-pounds, that is, 7,888 foot-pounds of work are performed by the engine during a movement of the piston equal to 0.61, while in the engine without compression a movement of 1.00 cubic foot only does 5,731 foot-pounds.

The compression engine during this portion of its stroke has converted the heat entrusted to it into work at twice the rate of the other engine. This is a great point. Any method which converts the heat into work with the utmost possible rapidity, by reducing the time of contact between the hot gases and the cylinder, saves heat and enables the

theory of the engine to be more nearly realized.

Taking all circumstances into consideration, it is certainly not over estimating the relative advantage of the compression engine to say that it will, under practical conditions give, for a certain amount of heat, three times the work it is possible to get from the engine using no compression.

It will not be necessary to discuss the theory of type 2 in respect of loss of heat to the sides of the cylinder, as it is not much used, and has hitherto failed to yield results in any way equal to type 3. It will be seen, however, from Fig. 3, that the conditions are not so favorable for a minimum loss of heat as in type 3.

The temperature from the moment of admission at *c*, to the point of cut-off at *e*, is kept constant at 1,537° Centigrade, so that the loss of heat must be great, both the surface exposed and the mean temperature being high. It is the less necessary to discuss this point in the slow combustion engine, as the pos-

sibility of using a hot cylinder and piston reduces the loss by attaining a temperature not far removed from the entering air.

It will be interesting to calculate the amounts of gas required by these three types under the supposed conditions, and for this purpose an analysis of Manchester gas, and also of London gas, has been used as the basis of calculation.

ANALYSIS OF MANCHESTER COAL GAS.
BY BUNSEN AND ROSCOE.

Hydrogen	45.58
March gas	34.90
Carbonic oxide	6.64
Olefiant gas or ethylene	4.08
Tetrylene	2.38
Sulphuretted hydrogen	0.29
Nitrogen	2.46
Carbonic acid	3.67
	100.00 volumes.

Of this gas 1 lb. at atmospheric pressure and 17° Centigrade measures 30 cubic feet, and evolves on complete combustion 10,900 heat-units Centigrade,

equivalent to 15,146,640 foot-lbs. 1 cubic foot of this gas will therefore evolve on complete combustion heat equivalent to $\frac{15,146,640}{30}$ = 504,888 foot-lbs.

To obtain an idea of the difference in heating power of the different gases, there is given here a recent analysis of London gas.

ANALYSIS OF LONDON COAL GAS.

	(A.)	(B.)
Hydrogen	50.05	51.24
March gas	32.87	35.28
Carbonic oxide	12.89	7.40
Olefines	3.87	3.56
Nitrogen	—	2.24
Carbonic acid	0.32	0.38

Taking the average of the two analyses, 1 lb. weight of this gas at atmospheric pressure and 17° Centigrade, measures 35.5 cubic feet, and evolves on complete combustion 12,500 heat-units Centigrade, equivalent to 17,370,000 foot-lbs., 1 cubic foot of this gas will therefore evolve, on

complete combustion, heat equivalent to $\frac{17,370,000}{35.5} = 489,268$ foot-lbs.

The difference between the heat evolved by these gases is but small. As Glasgow coal gas is of a high illuminating power, it will be richer in olefines, and the heat evolved per cubic foot will be somewhat greater. Taking 505,000 foot-lbs. as the amount of heat evolved by 1 cubic foot of coal gas, the result is probably very near the average to be obtained from the coal gas of most towns. The number of foot-lbs. required for 1 HP. for one hour are $33,000 \times 60 = 1,980,000$. It therefore follows that if the whole heat to be obtained from gas were converted into mechanical work, 1 HP. for one hour requires $\frac{1,980,000}{505,000} = 3.92$ cubic feet.

Now, taking the three types of engines, the amount of gas required by each to give 1 IHP. per hour would be as follows:

AMOUNT OF GAS REQUIRED BY THREE TYPES OF ENGINE.

Type 1. $\frac{3.92}{0.21}$=18.3 cubic ft. per HP. per hr

" 2. $\frac{3.92}{0.36}$=10.9 " " "

" 3. $\frac{3.92}{0.45}$= 8.6 " " "

If these engines be worked without loss of heat through the sides of the cylinders, but the expanding gases falling in temperature only through doing work, the above results would be obtained.

It is interesting to compare the consumption of gas by the engines in actual practice, to see in what order it stands. Results have not been obtained from engines of equal volume swept through by the piston, but it is at once seen that the order is in accordance with what is required by theory.

AMOUNT OF GAS CONSUMED BY THE THREE TYPES OF ENGINE HITHERTO IN PRACTICE.

1. Lenoir..95 cu. ft. per indicated HP. per hr.
   Hugon..85 " " " "
2. Brayton.50 " " " "
3. Otto....21 " " " "

For the Lenoir and Hugon engines the results of experiments by Mr. Tresca, of Paris, have been taken, as stated by Professor Thurston, corrected for an error into which he has fallen. He states the consumption of the engine to be 32 cubic feet per IHP. per hour, and then goes on to say that on the brake 4 HP. is obtained, while 8.6 is indicated. He has neglected to deduct from the gross indicated power in the cylinder, the pump resistance, and thus calculates the consumption on the gross indicated, instead of on the available indicated power. The available indicated power is not more than 5.2 HP., and the consumption is not less than 50 cubic feet per IHP. per hour.

For the "Otto" engine have been taken the figures given by Mr. F. W. Crossley. It is seen that the results are much what would be anticipated from the theory already developed. The difference between types 1 and 3 is greater than theory would indicate; but at the time the Lenoir engine was in use, the imperfection of the igniting arrangements and the rapid heating of piston, and consequently of the entering gases, made its action diverge much more widely from theory than in the case with the "Otto." The latter engine not only has the advantage of a better theoretical cycle, but the arrangements are of a nature to secure a greater perfection of action, and consequently a still closer approach to theory. An amount of about 18 per cent. of the heat used by it is converted into work, but only 3.9 per cent. by the Hugon engine.

In types 1, 2 and 3, which have been discussed, it has been assumed that in each case the expansion doing work was

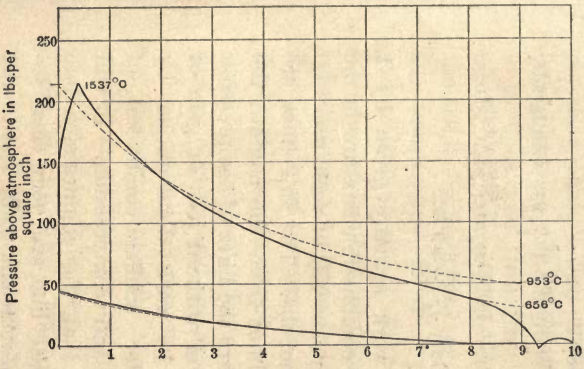
carried to twice the volume of the air before compressing.

Fig. 6 is a diagram from one of the author's engines which belongs to type 3. It will be observed that in this engine the expansion is only continued until the volume of the hot gases becomes equal to the volume before compression.

Taking the amount of work to be obtained from a cubic foot of air compressed to 40 lbs. above the atmosphere, and then heated to 1,537° Centigrade, expanding as the piston moves to its volume before compression, and then exhausting, it will be found to give the following results:

1 cubic foot of air (17° Centigrade and 760 milimeters mercury) at constant volume requires to heat it from the temperature of compression 150°.5 Centigrade to 1,537° Centigrade, heat equivalent to	24,416 foot-lbs.
Maximum pressure in lbs. per square inch above atmosphere.....	220 lbs.

Fig.6.



PARTS OF THE STROKE.

Diagram from Clerk's Gas Engine, cylinder, 6 ins. diameter, 12 ins. stroke. 150 revolutions per minute. Mean available pressure 70.1 lbs., 9 IHP. The maximum pressure is 220 lbs. per square inch above atmosphere. The pressure before ignition is 41 lbs. per square inch above atmosphere. The lower dotted line shows compression without loss of heat, to the same volume as exists in clearance space. Temperature before compresson 17°.3 C. (60° F.) Temperature after compression 150°.5 C. The upper dotted line shows the work done by air heated to 1,537° C., supposing it to lose no heat during expansion, except by doing work. The actual diagram shows a mean pressure during nine-tenth of stroke of 78 lbs. on the square inch, which is equal to 6,851 foot-lbs. per cubic foot of combustible mixture used. The dotted lines show an available pressure of 89.8 lbs. per square inch, which is equal to 7,888 foot-lbs. per cubic foot of air compressed. Duty$=\frac{7,888}{24,416}=0.323$.

Pressure at end of stroke in lbs. per square inch.....	49 lbs.
Mean pressure during available part of the stroke above atmosphere.....	89.8
Temperature at the end of the stroke.....	953° Centigrade.
Work done on the piston..	7,888 foot-lbs.

$$\text{Duty} \frac{7,888}{24,788} = 0.32$$

Now the work actually given by 1 cubic foot of combustible mixture in the author's engine, as will be seen from Fig. 6, is 6,851 foot-lbs. The full lines are the diagram lines from the engine; the dotted lines are the lines of compression and expansion without loss or gain of heat, except by work done on or by the air under similar conditions of temperature and compression. It will be observed that the compression line and the dotted line are very close together; no heat seems to be lost to the sides of the cylinder during compression; the loss of heat to the water-jacket is balanced by the gain of heat from

the piston, which must necessarily be much hotter than the cylinder sides, as it only loses heat by contact with the cylinder and by the circulation of air in the trunk. The temperature attained at the commencement of the stroke is in both cases identical, 1,537° Centigrade; the temperature at the end of the stroke without loss of heat is 953°; the temperature in the cylinder at the end of the stroke is 656° Centigrade. The diameter of the cylinder from which this diagram was taken is 6 inches, and the length of stroke 12 inches. This appears a very small loss of heat from a flame filling the cylinder, considering the surface exposed and the great difference of temperature between the ignited gases and the enclosing walls. Is it to be concluded, then, that the loss of heat to the cylinder during the time of the forward stroke is only 953°−656°=297° Centigrade? On this assumption the duty of the engine would be—

$$\frac{6,851}{24,416}=0.286,$$

and the consumption of gas per indicated HP. per hour would be—

$$\frac{3.92}{0.286}=13.7 \text{ cubic feet,}$$

but the consumption is 22 cubic feet per indicated HP. per hour, so that there has in some way been lost much more heat than is to be accounted for by the temperatures as determined by the diagram. The duty of the engine is—

$$\frac{3.92}{22}=0.178.$$

The duty of the engine expanding to the same volume as the mixed gases before compression is—

		Gas required per IHP. per hr.
		Cub. ft.
Duty without loss of heat to sides of cylinder.....	0.323	12.1
Duty with loss of heat as shown by diagram.....	0.286	13.7
Duty as determined by experiment.....	0.178	22.0

Now the number of cubic feet of combustible mixture required to produce 1

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HP. for one hour in the author's engine is—

$$\frac{1{,}980{,}000}{6{,}851}=289.$$

The amount of gas in the engine per cubic foot of mixture, $\frac{22}{289}=0.0761$ cubic foot, or $\frac{1}{13}$ of the total volume of gaseous mixture passed into the engine. If only the amount of gas necessary to heat the air to the required temperature is present, 1 cubic foot requires 0.0482 cubic foot of coal gas, or about $\frac{1}{21}$ of its volume; that is, although to heat up a cubic foot of inflammable mixture from 150° Centigrade to 1,537° only 0.0482 cubic foot of coal gas is required, yet although there is present 0.0761 cubic foot, or 1.58 time the amount necessary, the temperature does not rise any higher. Why is this?

Before going into the question, it is better to determine as nearly as possible what becomes of 100 heat units used by

the engine. The exhaust being discharged at a temperature of 656°, and the temperature of the air before compression being assumed at 17°, it follows that the exhaust from 1 cubic foot carries away with it $(656-17) \times 17.61 = 11,253$ foot-lbs.

The work done by the cubic foot of mixture is 6,851 foot-lbs., and the equivalent in foot-lbs. of the gas present in 1 cubic foot of explosive mixture is $0.0761 \times 505,000 = 38,430$ foot-lbs. The heat is therefore disposed of as follows:

	Foot-lbs.	Heat-units per cent.
Work done by 1 cubic foot of mixture	6,851	17.83
Mechanical equivalent of heat discharged with the exhaust	11,253	29.28
Mechanical equivalent of heat passing through sides of cylinder	20,326	52.89
	38,430	100.00

This investigation is only approximate. The determination, with anything like

possible physical accuracy, would require an examination of many points involving months of continuous work. It is the author's intention to make an accurate research into the phenomenon attending the use of the gas engine, for the purpose of obtaining the physical constants necessary to calculate exactly the consumption of any power, size, and theory of gas engine, such as it may be possible to construct in the future. For the present, however, it is only necessary to discuss the principles in such a manner us to clearly show where original research is required. More than one-half of the total heat given to the engine passes through the sides of the cylinder and is lost. How is this enormous loss of heat sustained, while only a comparatively small fall of temperature takes place below the adiabatic curve?

This leads back to the question of the gas present in excess of the amount necessary to raise the temperature to 1,537° which has already been noticed. At this point it is necessary to consider

the gas engine as something different from a hot-air engine.

The chemical phenomena attending combustion now require consideration. If 2 volumes of hydrogen be mixed with 1 volume of oxygen (the proportions necessary for complete combination of both gases to form water), and be ignited in a closed vessel in such a manner that the maximum pressure may be measured, it will be found that the pressure is a much lower one than would be expected if the complete combination of the two gases took place at once, and the whole heat due to this combination were developed. That this is not due to loss of heat to the sides of the vessel has been shown by Bunsen. He proved that the ratio of rise in pressure is exceedingly rapid compared to the rate of fall of pressure. The time taken for the inflammation of the whole volume of mixture is the time of attainment of the maximum pressure. In his experiments he used only a very small tube, which contained a volume of gaseous mixture,

8.15 centimeters long, by 1.7 centimeter in diameter, and the entire length of this column was traversed by the electric spark, in order that the inflammation of the whole mass in the tube might be as nearly instantaneous as possible. In practice he succeeded in producing a maximum temperature in so short a time as $\frac{1}{4000}$ part of a second. By examining the light from the explosion through a revolving disc provided with radiating segments, the rate of revolution of the disc being known, he determined the duration of light within the tube, and therefore the duration of a temperature not far removed from the maximum.

The duration of the illumination was found to be $\frac{1}{65}$ of a second. A maximum pressure, obtained in so short a time, with a duration so relatively long, makes it impossible that loss of heat through the sides of his tube could have affected his experiments. The cause, therefore, of the pressure falling so far short of what it would be if the combination took place completely. is simply this, that the

temperature is so high that complete combustion is impossible. The temperature, and therefore the pressure produced by the combination of any gases, is limited by the dissociation or decomposition of their products of combustion.

When any two gases combine, say (H) and (O) to produce water, what happens is this. The temperature rises till a point is reached, when any further rise would decompose the water which is already formed; and if the gases are kept at this temperature, no further combination will take place. If the temperature is lowered, further combination takes place until it is low enough to allow of the existence of steam without decomposition.

The temperature at which steam can exist as steam without its partial resolution into hydrogen and oxygen gases is not a high one. At 960° to 1,000° Centigrade Deville has proved that it commences to decompose, and at 1,200° Centigrade, considerable decomposition takes place, the amount of decomposition

increasing as the temperature rises: for each temperature there is a proportion of steam to free gases, which is constant, and does not change till the temperature changes. The same law holds true for carbon dioxide; at high temperatures it decomposes into carbonic oxide and free oxygen.

Bunsen attempted to determine the temperature attained on the explosion of a mixture of hydrogen and oxygen, a pure electrolytic mixture. He found that the maximum pressure attained by such a mixture is 10 atmospheres, the temperature before ignition being 5° Centigrade. From this he calculated the temperature produced, but in doing so, as Berthelot afterwards pointed out, he neglected the fact that when these gases combine, 3 volumes of the gases form 2 volumes of steam gas, and consequently if complete combination is assumed, and it be supposed that the pressure is produced by steam only, the volume, before ignition, must be calculated at two-thirds of that taken by the

mixed gases. But as it is known that combination is incomplete, at the lowest assignable temperature of the combustion, and it is not possible to tell the amount of combination at a given pressure without knowing the temperature, this cannot be assumed.

As in determining temperature by an air thermometer it is necessary that the amount of air in the thermometer should be constant at the different temperatures, it is evident that the temperature of an explosion cannot be known from the increase in pressure unless the chemical changes taking place do not alter the volume of gases under observation.

In calculating the temperatures attained in the author's engine, this fact has been kept in view. The capacity of the space at the end of the cylinder was carefully taken by filling with water and weighing the water. As the proportion of the combining gases to the excess of oxygen or free nitrogen is very small, only one-thirteenth of the whole volume used being combustible gas, the space

may be considered as simply filled with heated air, and the contraction caused by the formation of H_2O and CO_2 neglected, especially as an increase in volume follows the combination of the olefines with oxygen. 2 volumes of H combine with 1 volume of O, forming 2 volumes of steam. 2 volumes of marsh gas (CH_4) require for complete combustion 4 volumes of O, and form 4 volumes of H_2O and 2 volumes of CO_2. 2 volumes of carbonic oxide (CO) unite with 1 volume of O, forming 2 volumes of CO_2. If the olefines in coal gas be taken as of an average composition of C_2H_6, then 2 volumes require for complete combustion 9 volumes of oxygen, forming 6 volumes of H_2O and 6 volumes of CO_2.

Now taking the composition of coal gas as below the noted amounts of oxygen are required for combustion, and the given volumes of the products are formed—

vols.		vols.	vols.	
H=50	requires	25	O=50	H_2O produced.
CH_4=33	"	66	O=99	CO_4&H_2O "
CO=13	"	6.5	O=13	CO_2 "
C_3H_6= 4	"	18	O=24	CO_2&H_2O "

100 + 115.5=225.5 gives 186 vols.

The amount of contraction due to complete combustion of this coal gas is small even when burning with pure oxygen, 225 volumes of the mixed gases becoming 186 volumes after combustion. When diluted with nitrogen the proportion of contraction is less and introduces no serious error. With a mixture of 1 volume of gas to 12 volumes of air, 125 volumes of the mixture before combination become 122 volumes when completely combined, at the original temperature, assuming the water to remain gaseous. If the curve of the dissociation of water and carbonic dioxide were known, it would be possible to show on the indicator diagram the reserve of heat available at each point of the fall.

What the engineer requires of the

scientific chemist is a curve of the dissociation of water and carbonic acid, at temperatures ranging from the maximum produced by combustion down to the point at which it may be safely assumed that complete combination is possible.

In Fig. 6 the dotted line shows a fall of temperature, by hot air doing work without loss of heat through the cylinder, and the black line shows the actual fall of temperature in the author's engine, with loss of heat through the sides of the cylinder. It is evident then that the cause of so near an apparent approach to theory is, that at the maximum temperature, complete combination of gases with oxygen is impossible, and cannot take place until the temperature falls. As the temperature falls the gases further combine, until a temperature is reached at which combination is complete.

The loss of heat through the sides of the cylinder is therefore much greater than would appear from the diagram. In calculating the efficiency of the gas engine, all previous observers have assumed

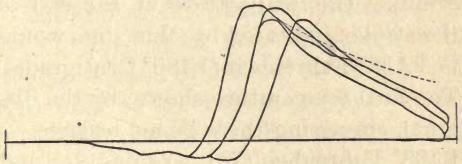
that the loss of heat to the cylinder is to be obtained from the comparison on the indicator diagram of the actual expansion-line with an adiabatic line from the same maximum temperature and pressure. So far as the author is aware, Professor Rücker, of Leeds, was the first to point out the necessity of taking into account the phenomena of dissociation in making such comparisons. Accordingly, all previous estimates of efficiency, based on the indicator diagram, are much too high.

The gas engine, then, differs from the hot-air engine, using air heated in the manner assumed in the first part of this paper, in this, that the temperature is sustained, notwithstanding the enormous flow of heat through the sides of the cylinder, by the continuous combination of the dissociated gases.

Figs. 7 and 8, have been taken from the "Journal of the Franklin Institute." They are Lenoir engine diagrams, and in them the same phenomena are apparent; although running at a very slow speed,

the pressure is most perfectly sustained, the dotted lines showing the adiabatic, and the full lines the actual diagram. The author of the paper in which they occur, gives the probable maximum tem-

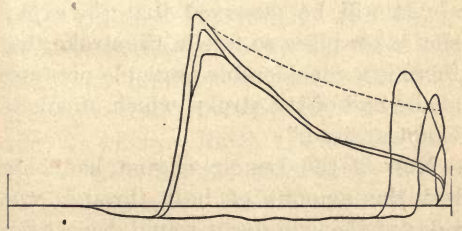
Fig.7.



LENOIR ENGINE.

Diagram at 50 revolutions, cylinder 8⅛ inches diameter, 16¼ inches stroke.

Fig.8.



LENOIR ENGINE.

Diagram at 45 revolutions, 1 inch=32 lbs.

perature attained at about 1,356° Centigrade, and he says, "The dotted line represents the theoretical curve of expansion, taking into account the loss of heat and consequent fall of pressure, due to the work done (which is the proper theoretical curve for an indicated diagram). The temperature at the end of the stroke, indicated by this line, would be 2,156° Fahrenheit (1,180° Centigrade). The final temperature shown by the diagram, supposing there be no leakage, is 1,438° Fahrenheit (781° Centigrade), and the difference 718° Fahrenheit (399° Centigrade), is the quantity of heat absorbed by the water-jacket by which the cylinder is surrounded."

"It will be observed that the explosion takes place so late in the stroke that there is a considerable available pressure at the end of the stroke, which of course is not utilized."

Now if the Lenoir engine had only lost this amount of heat through the sides of the cylinder it would have been very economical, and would have ap-

proached the theoretic consumption mentioned in the earlier part of this paper; but the causes of loss are so great that it never did come anything near this figure, and an error is introduced through neglecting the effects of dissociation.

Interesting information, however, is to be obtained from these diagrams as to the proportion of gas and air in the mixture used by the Lenoir engine. When these diagrams were taken the maximum temperature after ignition was 1,356° Centigrade; now in the author's present engine the maximum temperature is 1,537°; it follows that Lenoir used a more diluted mixture as the temperature after ignition was lower. The engine giving this diagram could not have been using an ignitable mixture containing more gas than one-fourteenth of its volume—a mixture which the author finds to be easily ignited at ordinary atmospheric pressure. The statement is often made that such a mixture will not explode except it be first compressed; this

is incorrect, it is possible to ignite even a weaker mixture without compression. Coquillon has determined the limits between which a mixture of marsh gas (CH_4) and air can be exploded. Mixtures of marsh gas and air in different proportions were introduced into a eudiometer and fired by the electric spark, with the following results:

Marsh gas 1 volume, air 5 volumes. The spark is without effect. Marsh gas 1 volume, air 6 volumes. Explosion only occurs in a succession of shocks. This is the first limit of possible explosion; the marsh gas is in excess. Marsh gas 1 volume and 7, 8 and 9 volumes of air give a sharp explosion. With 12, 13, 14, 15 volumes of air for 1 volume of marsh gas the explosion occurs, but grows gradually weaker. With 16 volumes of air the effect is reduced to a series of slight intermittent commotions. This is the second limit; the air is in excess.

In Fig. 8, ignitions will be observed very late in the stroke; these misses were caused by the points between which

the electric spark is discharged getting wet and thus preventing the passage of the spark at the proper time. From these diagrams, the time, from the beginning of rise in pressure to the attainment of maximum pressure, is found to be from one twenty-seventh to one-thirtieth of a second; when the ignitions are late it takes longer, one-twentieth of a second being required; that is, the flame has spread completely through the mass in one-twentieth part of a second.

Now in the author's engine, calculating from the moment when the ignition port is opening to the flame, to the moment of maximum pressure as found from the diagrams, it has been ascertained that the time occupied is an average of one twenty-fifth of a second, a time nearly identical with that found for the Lenoir engine.

If it be admitted that the flame has spread completely through the mass when the maximum pressure is attained in the Lenoir engine, it cannot be supposed that it has not spread in like man-

ner throughout the mass of ignitable mixture in the modern compression engine. Maximum pressure is the only outward indication of complete inflammation; by complete inflammation is not meant the thorough chemical combination of the active gases present, but the spread of the flame through the entire mass. That when maximum pressure has been reached complete inflammation has also been attained has hitherto been considered self-evident. It is only lately that the theory has been advanced by Mr. Otto that in the modern compression engine attaining maximum pressure at the beginning of the stroke, the flame has not spread throughout the mass of the ignitable mixture in the cylinder; but that as the piston moves forward the pressure is sustained by the gradual spread of the flame. This supposed phenomenon has been erroneously called slow combustion; if it has any existence it should be called slow inflammation. It has a real existence in the Otto engine only when it is working badly; but even then maximum temper-

ature is attained, and very distinctly marks the point of completed inflammation.

The time taken to attain maximum pressure is longer in a large engine than in a small one, because the distance through which the flame has to travel is greater. During the investigation already referred to, Professor Bunsen determined the celerity of the propagation of ignition through a pure explosive mixture of hydrogen and oxygen in the following manner: the explosive mixture was allowed to burn from a fine orifice of known diameter, and the current of the rate of the gaseous mixture was carefully regulated by diminishing the pressure, to the point at which the flame passed back through the orifice and ignited the gases below it. This passing back of the flame occurs when the velocity with which the gaseous mixture issues from the orifice is inappreciably less than the velocity with which the inflammation of the upper layers of burning gas is propagated to the lower and unignited layers.

The rate of the propagation of the ignition in pure hydrogen was found to be 34 meters per second. In a maximum explosive mixture of carbonic oxide and oxygen it was not quite 1 meter per second.

Mr. Mallard has determined the rapidity of the propagation of inflammation through mixtures of coal gas and air by this method, and found that the maximum rate of propagation was attained with a mixture of 1 volume of coal gas with 5 volumes of air, and it is 1.01 meter per second. One volume of coal gas with $6\frac{1}{2}$ volumes of air gave a rate of 0.285 meter, or 11 inches per second.

This is the rate of ignition, it must be remembered, at constant pressure; in a closed tube fired at one end it would ignite with much greater rapidity. In a closed space the conditions of inflammation are quite different. The ignited portion instantly expands, compressing the portion still remaining, and thus carries the flame further into the mass, so that to the rate of ignition at constant

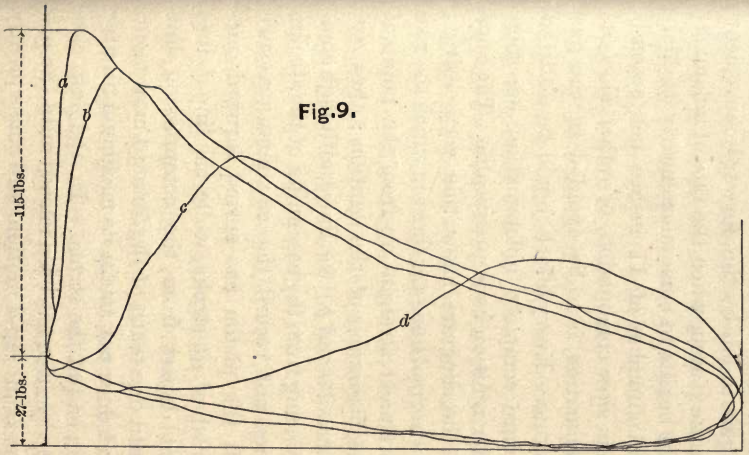
pressure is added the projection of the flame into the mass by its expansion. To determine from the rate of ignition at constant pressure the time necessary to completely inflame a given volume of mixture at constant volume is a very complicated problem, which it is probable can only be solved experimentally.

The author has found it possible to ignite a whole mass in any given time between the limits of one-tenth and one-hundredth part of a second, by so arranging the plan of ignition that a small volume of gaseous mixture is first ignited, expanding and projecting a flame through a passage into the mass of inflammable mixture, and thus adding to the rate of ignition the mechanical disturbance produced by the entering flame. He has succeeded by this means in producing maximum pressure in one-hundredth part of a second in a space containing 200 cubic inches. This rate of ignition is too rapid, and would not give the engine time to take up the slack in bearings, connecting rods, &c. But by firing a

mixture with varying amounts of mechanical disturbance almost any time of ignition can be obtained between $\frac{1}{100}$ and $\frac{1}{10}$ of a second. It does not matter whether the mixture used is rich or weak in gas; the rich mixture can be fired slowly and the weak one rapidly, just as may be required. The rate of ignition of the strongest possible mixture is so slow that the time of attaining complete inflammation depends on the amount of mechanical disturbance permitted.

Fig. 9, a diagram from an Otto engine, shows what happens in a compression engine of type 3 when the ignition comes late and the movement of the piston overruns the rate of the spread of the flame. It is then seen that the maximum pressure is not attained until far on in the stroke, and as a consequence great loss of power results, the pressure attaining its maximum when it is time for the exhaust valve to open. This may happen from several causes, a too diluted mixture, or too little mechanical disturbance by the entering flame; or the igni-

Fig.9.



COMPRESSION GAS ENGINE.

tion may be missed until the pressure begins to fall by the forward movement on the piston, when the rate of inflammation begins to come more nearly to Mallard's number of 11 inches per second. This slow combustion, or rather slow inflammation, is to be avoided in the gas engine. Every effort should be made to secure complete inflammation as soon after ignition as is practicable. The lines in the diagram show this very clearly; the normal lines are those in which the rise is almost straight up from the point of the beginning of the ignition; they are marked *a* and *b*; the line *c*, although commencing from the beginning of the stroke, does not record the maximum pressure till the piston has moved forward one-third of its stroke, while the line *d* does not depart from the compression line until one-tenth of the forward movement, and does not attain its maximum till near the end of the stroke. In the last case the ignition has been missed until the piston is in rapid motion, and consequently the flame is at first unable to overtake it.

The rate of inflammation at constant pressure has been determined only for atmospheric pressure; were it known for higher pressures it would be possible to calculate exactly the piston speed which would prevent any rise in pressure at all.

Fig. 10 was taken by the author from the motor cylinder of an American Brayton engine of type 2. It shows how the pressure is sustained as the ignited gases enter the motor cylinder in flame. This is the true slow inflammation engine; in it the pressure after ignition is not allowed to rise, but only increase of volume takes place; at about the middle of the stroke the supply of flame is cut off and the piston moves on, and the heated gases expand doing work.

Fig. 11 is the compression pump diagram, which must be deducted before getting the available indicated power. The motor-piston was of the same area as the pump, but had double the length of stroke. This type of engine is not a good one for a cold cylinder, the loss of heat through the cylinder being much

more than in type 3; but, as it has been before said, the possibility of using the

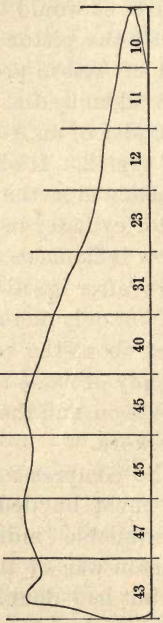


Fig. 10.

BRAYTON PETROLEUM ENGINE (MOTOR CYLINDER).
Area of piston, 50.26 inches. Stroke, 12 inches. Mean pressure, 30.2 lbs.

theory in the future with a hot piston and cylinder renders reference to this engine

interesting. Slow inflammation is a mistake if applied to engines of types 1 and

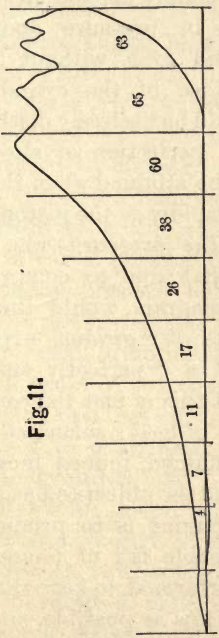


Fig. 11.

BRAYTON PETROLEUM ENGINE.

Air-pump diagram. Area of piston, 50.26 inches. Stroke, 6 inches. Mean pressure, 27.6 lbs. Pressure in reservoir, 60 lbs.

3 with cold cylinders; in type 1, if the piston were moving rapidly enough, the inflammation could be so slow that with

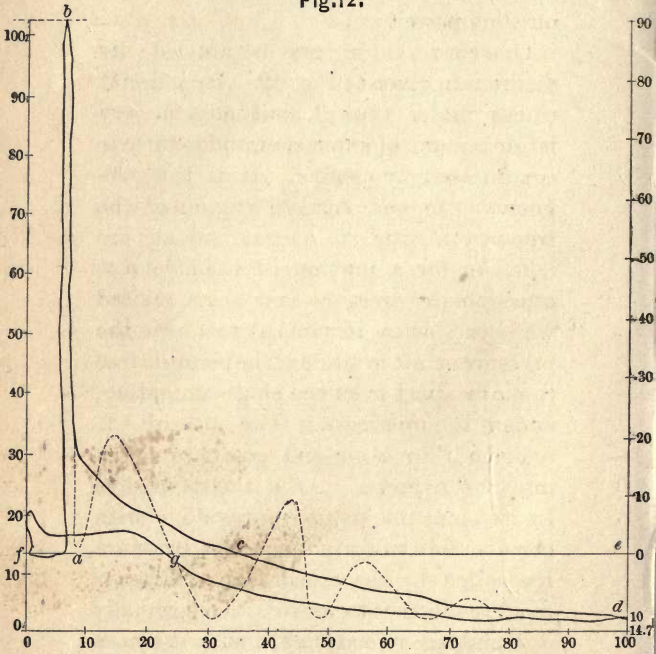
a perfectly sustained temperature no power at all could be obtained. That is, the air would simply expand in volume without rising in pressure above the atmosphere, and even without loss of heat to the sides of the cylinder the whole heat would be uselessly discharged. In type 3 the perfection of slow combustion would be attained when the flame spread just as rapidly as the piston moves forward, and the pressure was never raised above that due to compression. The pressure diagram would then give the ideal results of "gradual expansion of gases" and a "perfectly sustained pressure." But this is just the condition of greatest loss of heat; sustained pressure means sustained, indeed increasing temperature, and the object to be attained in a good gas engine is to produce the most rapid possible fall of temperature due to work performed, to keep the mean temperature as low as possible, and it is only so far as this is successfully done that economy is possible. Slow inflammation causes loss of heat and power;

rapid inflammation reduces the loss to a minimum while attaining the maximum possible power.

One more engine may be noticed; its diagram is given at Fig. 12. In action it comes under type 1, but uses a very large amount of expansion, and is further complicated by cooling. It is the well-known Otto and Langen engine of the free piston type; in it gas and air are taken in for a portion of the stroke at atmospheric pressure and then ignited while the piston remains at rest until the pressure sets it in motion; the piston is free to move apart from the shaft altogether, and on the up-stroke it does no work.

From *f* to *a* air and gas are taken into the cylinder. At *a* the mixture is ignited and the piston moves to *c* with considerable velocity when the pressure has fallen to the atmosphere. From *c* to *e* it continues to move with continually diminishing velocity, until at *e* it comes to rest and then returns doing work, the work being equal to the diagram *d g e* added to the weight of the piston and

Fig.12.



OTTO AND LANGEN ENGINE (FREE PISTON).

Percentage of stroke.

rack through the stroke. It will at once be seen that as the gases only do work on the piston from *a* to *c*, and this work is absorbed in giving a certain velocity to the piston, and from *c* to *e* the velocity of the piston is being gradually checked by doing work on air, assuming the piston to have no weight, the area of the portion of the diagram *a c b* must be equal to the part *c e d*.

It is evident that the lines in the diagram are incorrect; the explosion cannot fall nearly so rapidly as shown; *c* should be much nearer *e*. The oscillations of the indicator have been so great that accuracy is impossible. The fall of the line *d g* below *d e* is caused by the cooling of the gases on the return stroke. In this engine the advantage consists more in the large amount of expansion than the velocity of the forward movement of the piston.

The diagram has been taken from a paper by Mr. F. W. Crossley; with reference to it he says:

"The very sudden and extreme rise in

pressure at the moment of explosion is due simply to the expansion of the gases under the temperature of the flame. If this temperature be taken at 5,000° Fahrenheit, and divided by 520 for the rate of expansion from an initial temperature of about 60°, it gives an expansion of about 10 times; and as the gas compound occupied one-eleventh of the cylinder at the moment of ignition, if it expands ten times it gives very nearly the stroke actually taken by the piston. The 5,000° is an assumption only, but seems to be confirmed by the amount of expansion which follows it. After the explosion the temperature falls almost instantaneously, as shown by the sudden drop of pressure in the diagram."

In the author's opinion Mr. Crossley has completely misinterpreted his diagram. Taking the temperature before ignition at 60° Fahrenheit, and the maximum pressure shown on the diagram as 100 lbs. absolute, it follows that the maximum temperature is not greater than 2,900° Fahrenheit (1,590° Centi-

grade). It is difficult to see how 5,000° Fahrenheit can be assumed. The expansion of the gases by the extreme movement of the piston following ignition has no necessary relation to the temperature of the explosion; but it is determined wholly by the work done on the piston by the explosion between the maximum and atmospheric pressures. Whenever the gases in the cylinder fall to the pressure of the atmosphere, which happens according to the diagram at about 0.35 of the stroke, the piston is doing work on air, and the mean pressure below the atmosphere from *c* to *e* is the exact measure of the work previously done on the piston by the explosion, which has been expended in giving the piston velocity. This energy of motion is now being expended by compressing the atmosphere. Taking into consideration the weight of the piston and friction of the rings, rack and clutch, it is certain that the area of the part of the diagram *a b c* must be considerably greater than *c e d*; in the diagram it ap-

pears much less. It should be greater by the amount of work expended in giving the piston energy of position, and the amount lost by friction on the up-stroke.

As a means of showing the nature of the explosion this diagram is misleading; it is certain that the maximum pressure was less, and that the fall of pressure is nothing like so rapid as it there appears. Comparing Fig. 12 with Figs. 7 and 8 the difference in appearance is so striking that it looks as if in one case the fall in pressure was instantaneous and in the other gradual; this would be remarkable, considering that the maximum temperatures are very similar. If the lines in Fig. 12 be corrected and drawn with the same relation of scale between pressures and strokes, it will be found to be very similar in appearance to Figs. 7 and 8, so far as rate of fall is concerned. Indeed the advantage claimed for this engine is a movement of piston so rapid that its expansion is complete before much heat is lost to the sides of the cylinder, which is inconsistent with a

fall of pressure more rapid than in the Lenoir engine.

To go completely into the points of originality in these engines would require a paper on the "History of the Gas Engine;" but it may be well to state the name of the first to propose each type:

		Year.
Type 1.	Explosion acting on piston connected to crank...W. L. Wright	1833
	Explosion acting on free piston, Barsanti & Matteuci	1857
Type 2.	Compression after ignition but at constant presssure. C. W. Siemens	1860
	Compression with increase in volume.....F. Millon	1861
Type 3.	Compression with increase in pressure.....F. Millon	1861
	After ignition but at constant volume.....	

So far as the author has been able to ascertain, these are the names of the first to propose distinctly each of the three types of gas engine.

From the considerations advanced in the course of this paper, it will be seen that the cause of the comparative efficiency of the modern type of gas en-

gines over the old Lenoir and Hugon is to be summed up in one word, "compression." Without compression before ignition an engine cannot be produced giving power economically and with small bulk. The mixture used may be diluted, air may be introduced in front of gas and air, or an elaborate system of stratification may be adopted, but without compression no good effect will be produced.

The proportion of gas to air is the same in the modern gas engine as was formerly used in the Lenoir, the time taken to ignite the mixture is the same, the only difference is compression. The combustion, or rather the rate of inflammation, is indeed quicker in the modern engine because the volume of mixture used at each stroke is greater, and yet the time taken to completely inflame the mixture is no more than in the old type. The cause of the sustained pressure shown by the diagrams is not slow inflammation (or slow combustion as it has been called), but the dissociation of the

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products of combustion, and their gradual combination as the temperature falls, and combination becomes possible. This takes place in any gas engine, whether using a dilute mixture or not, whether using pressure before ignition or not, and indeed it takes place to a greater exsent in a strong explosive mixture than in a weak one.

The modern gas engine does not use slow inflammation (or slow combustion if the term be preferred), but when working as it is intended to do, completely inflames its gaseous mixture under compression at the beginning of the stroke. By complete inflammation is meant complete spread of the flame throughout the mass, not complete burning or combustion. If by some fault in the engine or igniting arrangement the inflammation is a gradual one, then the maximum pressure is attained at the wrong end of the cylinder, and great loss of power results.

Compression is the great advance on the old system; the greater the compression before ignition the more rapid will

be the transformation of heat into work by a given movement of the piston after ignition, and consequently the less will be the proportional loss of heat through the sides of the cylinder. The amount of compression is of course limited by the practical consideration of strength of the engine and leakage of the piston, but it is certain that compression will be carried advantageously to a much greater extent than at present. The greatest loss in the gas engine is that of heat through the sides of the cylinder, and this is not astonishing when the high temperature of the flame in the cylinder is considered. In larger engines using greater compression and greater expansion it will be much reduced. As an engine increases in size the volume of gaseous mixture used increases as the cube, while the surface exposed only increases as the square, so that the proportion of volume of gaseous mixture used to surface cooling is less the larger the engine becomes. Taking this into consideration, it may be accepted as probable that an

engine of about 50 indicated HP. could be made to work on 12 cubic feet of coal gas per indicated HP. per hour, or a duty of about 32 per cent.

The gas engine is as yet in its infancy, and many long years of work are necessary before it can rank with the steam engine in capacity for all manner of uses; but it can and will be made as manageable as the steam engine in by no means a remote future. The time will come when factories, railways amd ships will be driven by gas engines as efficient as any steam engine, and much more safe and economical of fuel. Grs generators will replace steam boilers, and power will not be stored up in enormous reservoirs, but generated from coal direct as required by the engine.

The steam engine converts so small an amount of the heat used by it into work that, although it was the glory and honor of the first half of the century, it should be a standing reproach to engineers and scientists of the present time having constantly before them the researches of Mayer and Joule.

APPENDIX.

DATA USED IN THE PAPER ON "THE THEORY OF THE GAS ENGINE."

Specific heat of air at constant volume.	= 0.169 ; water 1.00
Specific heat of air at constant pressure	= 0.238
Mechanical equivalent of heat foot-lbs. Centigrade.....	=1389.6
Specific heat of air at constant volume in foot-lbs. for 1 cubic foot at 17° C. and 760 mm. barometer..... .	17.6 foot-lbs.
Specific heat of air at constant pressure in foot-lbs. for 1 cubic foot from 17° C. and 760 mm.....	24.8 "
Weight of 1 cubic ft. of air at 17° C. and 760 mm.....	0.075 lb.

Burning completely in oxygen, the following substances are taken as evolving the noted amounts of heat in Centigrade units, per unit weight of substance burned.

Hydrogen.....	34,170
Carbon..	8,000
Carbonic oxide	2,400
Marsh gas.....	13,080
Olefiant gas.....	11,900

DISCUSSION.

Mr. D. CLERK mentioned that Dr. Siemens had worked out the method of compression used in engine type 2 in 1860 in so complete a manner that no advance had since been made on it by any one. Dr. Siemens was again working at this type of engine, which, from the fact of it using hot cylinder and regenerator, Mr. Clerk was certain was the best type for the very large gas engines to be developed in the future. With respect to the cold cylinder engine, of which alone he had treated in the paper, he wished again to insist on this: that the theory which sought to explain the so-called sustained pressure on the indicated diagram by the hypothesis of slow inflammation (erroneously termed slow combustion) was a false one. That when maximum pressure was attained in the gas engine cylinder it was certain that the whole mass was completely inflamed, and that no system of stratifica-

tion producing slow inflammation could do good, but was quite opposed to the conditions of economy.

Dr. Siemens said that one part of the paper dealt with matters regarding the mechanical arrangement of gas engines, and the other with a theoretical question, that of the law of combustion. He would refer to the theoretical part first, because the author appeared to attach great importance to it, and as Dr. Siemens had from time to time given a great amount of consideration to the action of negative combustion or dissociation, it might be of some interest to the members to see how far his views fell in with those set forth by the author. It was well known that by combustion no unlimited degree of temperature could be attained. Thus, in a furnace worked at very high temperature the fuel was not completely burned when it came in contact with the oxygen of the heated or non-heated air. The moment a certain comparatively high temperature was reached the carbon refused to take up oxygen, or the hydrogen

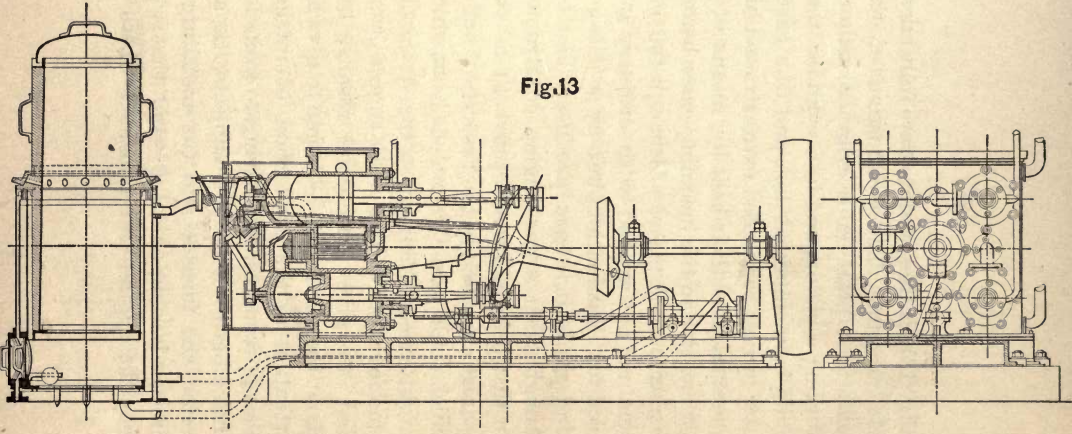
refused to take oxygen, and what had been called by Bunsen, and, shortly after him, by St. Claire Deville, dissociation, arose. The point of dissociation was not a fixed one; partial dissociation came into play at a comparatively low temperature, and went on increasing at a higher temperature in very much the same ratio as vapor density increased with temperature. Thus, if aqueous vapor were passed through a tube at a sufficient temperature the whole of the vapor would be dissociated, and the oxygen and the hydrogen would be separated. It was true, if these gases were left to themselves they would, the moment the temperature lowered again associate or burn; but if precautions were taken to cool them rapidly after they had attained that high temperature they would be found as a mixture of oxygen and hydrogen simply. The author had stated that the law which governed these actions was not well known and required research, but Dr. Siemens would like to know whether he was aware of the researches

of St. Claire Deville on the subject. It might be that the determinations of St. Claire Deville were not quite correct, but in the meantime they might be regarded as being so. He found that at atmospheric pressure the point of half dissociation of aqueous vapor arose at a temperature of 2,800° Centigrade, and that of complete dissociation at a much higher temperature. Taking that law as determined by the French philosopher, it did seem reasonable to suppose that when a mixture of hydrogen and oxygen, with or without a mixture of nitrogen exploded, the point was reached beyond which the temperature did not increase, and, according to the author, that point was 1,500° Centigrade. If such a temperature was reached in a working cylinder complete combustion would not take place immediately, but only partial combustion would occur, which would go on as the temperature diminished by absorption into the cylinder or by expansion, and that combustion would be completed only in the course of the stroke. In that

way the action which had been described with reference to the diagrams was reasonable enough. With regard to the mechanical arrangement of gas engines, the author distinguished between three types. In the first, the mixture of gas and air drawn in at atmospheric pressure was exploded. In the second, with which the author had connected his name as that of the first proposer, the combustion was produced gradually; the gases were ignited as they flowed into the heating cylinder. In the third type, the gases, after being compressed and mixed, were admitted into the working cylinder, and suddenly exploded. With reference to the early engine which Dr. Siemens constructed in 1860, the author had stated that it combined other elements, which were entirely wanting in the gas engines of the present day. The gas engine of the present day, taking either of the three types, was, in his opinion, in the condition of the steam engine at the time of Newcomen. The fuel was burnt in a cylinder which it was attempted to keep

cold by passing water over it, and it was easy to conceive that the heat so generated, was only partly utilized for maintaining the state of expansion of the heated gases, the cold sides of the cylinder taking a good half of it away at once, thus causing a great loss. Then there was another palpable loss in these engines. After expansion had taken place, after half the heat had been wasted in heating a cylinder which was intended to be kept cool in order to allow the piston to move, the gases were discharged at a temperature of 1,000°, or in the best types about 700°. That amount of heat, representing in one case one-half and in the other two-thirds of the total heat generated, was thrown away. This was heat which could be saved and made useful. Instead of commencing the combustion at a temperature of 60°, if the heat of the outgoing gases were transferred to the incoming gases, combustion might commence at a temperature of nearly 1,000°, and the result would be a very great economy. In the engine which he constructed in 1860

Fig.13



(Fig. 13) all those points were fully taken into account. The combustion of the gases took place in a cylinder without working a piston, and in a cylinder that could be maintained hot, and the gases after having completed expansive action, communicated their heat by means of a regenerator to the incoming gases before explosion took place. Although the engine was not worked with ordinary gas used for illumination, but by a cheaper kind made in a gas producer, he then thought that a gas engine constructed on that principle would prove to be the nearest approach to the theoretical limits which could never be exceeded, but which might exceed the limits of the steam engine four or five fold. The engine promised to give very good results, but about the same time he began to give his attention to the production of intense heat in furnaces, and having to make his choice between the two subjects, he selected the furnace and the metallurgic process leading out of it; and that was why the engine had remained where it

was for so long a time. But now the time had come when there was a greater demand for engines of a smaller kind to do their best in houses and in small works, and when marine engineers especially had become fully alive to the importance of more economical arrangements. He therefore looked upon the question before the Institution as one of first importance to engineers, and he hoped that it would be well discussed.

Professor Rucker said that in his work on Thermodynamics, Mr. Verdet had published a calculation of the theoretical efficiency of an ideal gas engine. He assumed that no heat was lost through the sides of the cylinder, and that the explosion was so sudden that the whole of the gas was inflamed before the piston had appreciably moved; and under those circumstances he found that if the gases used were carbonic oxide, and a sufficient quantity of air to burn it completely, and if the whole of the carbonic oxide was burnt, the temperature to which the gases would rise, on the assumption that their

specific heats remained constant, was 4,388° Centigrade. He found that the pressure would rise from 15 lbs. per square inch to 215 lbs., and that the efficiency of the engine would be 41 per cent.—that was, that 41 per cent of the total amount of heat produced by combustion of the gas would be converted into useful work. It was evident from the conditions of Mr. Verdet's problem that that was a purely theoretical calculation. The condition, for instance, that no heat was lost was one which could not be realized in practice. About four years ago, however, in the course of a series of lectures given by some of his colleagues and himself on coal, he pointed out that Mr. Verdet's calculation was not even theoretically correct; that Bunsen had proved that it was impossible that a mixture of carbonic oxide and air could reach such a temperature as 4,388° Centigrade, which was something like 2,800° above the highest temperature, which Berthelot had shown was consistent with Bunsen's experiments on the subject.

The question then arose what the effect of dissociation would be upon the gas engine, and Professor Rucker attempted to make a rough calculation to show how important it might be. In the first place, he assumed that the highest temperature which could be reached was that given by Bunsen's experiments, and in the next that the specific heats were constant and the inflammation instantaneous. With those conditions only about one-half of the carbonic oxide would be burned when the highest temperature was reached; then, as the piston began to move forward and the temperature fell, more would be consumed. But then there was the very important question as to how the temperature would fall, and in order to calculate that the law of cooling of a body heated to that extremely elevated point must be known. That, of course, he was ignorant of, and he was therefore obliged to make a rough assumption. Assuming that, as the piston moved forward, the gas burned so as to keep the temperature

constant, he found that at the end of the stroke, when the pressure had fallen to that of the atmosphere, a part of the gas was left still unconsumed. Therefore in the half of the gas left unburned to begin with, there was sufficient to do all work that was done while the piston was moving forward. The only assumption he could make was that the temperature remained constant; any other, though that certainly was not true, would have involved some still more arbitrary hypothesis as to the law of cooling. Making, then, that rough assumption, he found that instead of a temperature of 4,000° Centigrade the highest reached would be about 2,000°; that the pressure, instead of rising to 215 lbs., would rise only to 103 lbs.; and that the efficiency of the engine would be only 25 instead of 41 per cent. That, though a very rough calculation, showed at once what the enormous importance of the phenomenon of dissociation might be. It served the purpose for which it was put forward, and

showed that in any theory of the gas engine physicists must make up their minds as to what part dissociation played in it. Passing from the theoretical problem to that Mr. Verdet and himself discussed, namely, the case in which there was only enough air to burn the carbonic oxide completely, to the practical problem in which there was a much larger quantity of air present, a case arose in which dissociation was less important. The larger the quantity of air present the lower the highest temperature would be, and therefore, probably, the smaller the amount of dissociation. St. Claire Deville had shown that carbonic acid was dissociated at temperatures between 1,000° and 1,200°, and water at temperatures between 1,000° and 1,100° Centigrade. Inasmuch, therefore, as in the author's engines, the highest temperature reached was about 1,500° (or 400° or 500° above the limits put by St. Claire Deville), it followed that if his measurement of the temperature was correct,

which there was every reason to believe it was, and if St. Claire Deville's experiments were trustworthy, there was a certain amount of dissociation at the temperatures reached in his gas engine. Passing, however, to the next question, namely, how much dissociation there was, the problem was much more difficult. With regard to that subject a series of papers had recently appeared in the "Comptes Rendus de l'Académie des Science," which were so much to the point that he might be excused for giving a short account of one or two of the leading results at which the experimenters had arrived. The two gentlemen in question were Mr. Mallard (whose experiments on the rate of propagation of inflammation in gas had been mentioned by the author) and a colleague, Mr. Le Chatelier. They had been making a number of experiments such as those that the author had advocated in his paper. They had made, indeed, what appeared to be one of the first serious attempts to investigate what was going

on in gas heated between 1,000° and 1,500° Centigrade. The plan they adopted was as follows: They exploded gases in an iron cylinder, attached to which was a Bourdon manometer; to that was attached a needle, which registered the pressure on a revolving cylinder. By reading off the curve so obtained, they got information as to the pressure in the cylinder at different times. He could not altogether accept their results without further confirmation. Some of the conclusions at which they had arrived were so striking that he thought they must certainly be supplemented by other experiments before they could be accepted. But for the moment he would put aside all difficulties connected with the experiments, and simply state the conclusions. It was found, dealing with gases at very different temperatures, that the curves obtained upon the revolving cylinder showed a point of discontinuity. At the very highest temperatures the curves were somewhat different from what they were at low temperatures, and

the assumption they made was that at the high temperatures dissociation had set in, whereas at the lower temperatures there was no dissociation; therefore the law of cooling would be different in the two cases. If, however, that interpretation of the experiments was accepted, it would be found that the temperatures at which dissociation took place to any considerable extent were higher than those he had mentioned. Thus the authors stated that carbonic acid did not dissociate appreciably below 1,800° Centigrade, and that steam-gas did not dissociate appreciably below 2,000°. Here, then, there were temperatures considerably above those obtained in the gas engine; if, therefore, the results in question were to be accepted, dissociation could not play a very important part in the matter. But although at first sight the experiments told against dissociation taking place to any large extent, in order to account for the phenomena they observed, Messrs. Mallard and Le Chatelier had had to introduce another hypothesis which practi-

cally came to very much the same thing. In all the earlier calculations upon the subject the assumption had been made that the specific heats of the gases were the same at high as at very low temperatures, but within the last few years two or three experimentalists of note had brought forward results tending to show that the specific heat of the gases increased as the temperature rose. The two most important researches made upon the subject were those by Professor E. Wiedemann and Professor Wüllner, the latter of whom showed that at temperatures between zero and 100° Centigrade there was an appreciable rise in the specific heat of gases at a constant volume. Messrs. Mallard and Le Chatelier had taken that hint, and they found that in order to explain the facts observed by them on the assumption that there was no dissociation, they must assume an enormous increase in the specific heats of the gases at high temperatures. But there were one or two points which appeared to present difficulties in

their way. Wüllner showed that at the temperatures at which he worked, as might be *prima facie* expected, the increase was much greater in a compound gas like water or carbonic acid than in an elementary gas such as oxygen or nitrogen. But Messrs. Mallard and Le Chatelier completely reversed that, and found that the increase was much greater in the elementary gases than in the compound ones; and they went so far as to show that oxygen would at a temperature of 1,000° have a specific heat no less than one hundred and sixty-five times greater than that which it had at zero. That result was so astonishing that it could not be accepted without much more proof than had at present been offered. But putting aside for the moment Messrs. Mallard and Le Chatelier's interpretation of the experiments, he wished to consider what they meant from a wider point of view, viz, that those gentlemen had come across a phenomenon which pointed to the fact that a vast quantity of heat was ren-

dered latent. If specific heat at constant volume increased, the meaning of it must be that the work done by the heat was done within the molecules of the gas, that the heat was spent in separating or preparing for separation the atoms of those molecules, which were gradually being forced asunder; whether they were actually forced asunder or not might be a question, but a large amount of work was spent in separating them, or preparing to separate them, by loosening the bonds between them; and Messrs. Mallard and Le Chatelier's experiments served as much as anything previously brought forward to illustrate that point. He thought it must be assumed with almost certainty that a large quantity of heat was rendered latent in gases at temperatures between 1,000° and 1,500° Centigrade. All would agree that a certain amount of that heat was spent in dissociation (for Messrs. Mallard and Le Chatelier stated that they harmonized their results with those of St. Claire Deville by supposing that his experiments

were more sensitive than their own), and the remainder of the heat would be spent, if not actually in dissociation, in preparing for dissociation. There was one other point in the paper which he thought of interest. The author had pointed out how different the rate of propagation of an explosion would be in the case of gaseous mixture which was confined to that in an unenclosed space. Messrs. Mallard and Le Chatelier had made experiments on that point; they had inflamed gas and air mixture in a tube closed at one end, and they found that when it was inflamed at the closed end the rate of propagation was much greater than when it was inflamed at the open end. In the one case the gas was merely burning backwards through the tube, in the other the expansion of the gases would spread the inflammation. So enormous was the difference, that in some cases they found that the rate of propagation was one hundred times greater when the gas was lighted at the closed end of the tube than when it was

lighted at the open end. That was a point which strongly confirmed the author's view—that inflammation spread through the gas almost instantaneously. Although, therefore, one could not but feel that on those points there was a great lack of experimental data, all the facts that were brought together, might, at present, be best explained by the hypothesis that the inflammation spread very rapidly through the gas, and that at high temperatures, say of over 1,000°, a very large amount of heat was rendered latent, either in actual dissociation or in incipient dissociation. Here, then, was an explanation of the curious maintaining of the temperature to which the author had referred. As the gas cooled, the latent heat was given up and the curve was thus kept up to a high temperature by the heat previously absorbed in the molecules of the gas.

Mr. W. R. Bousfield did not propose to quarrel with the greater part of the facts stated, which were for the most part indisputable, but he thought neither

the interpretation which the author had put upon them could be upheld, nor the new and, to most of them, rather startling theory of the action of the gas engine which had been submitted in the paper. He did not say that the phenomena of dissociation played no part in the action of the gas engine; he did not say that when the explosion took place, there might not be a certain quantity of ammonia and a certain quantity of nitric acid formed, and that the phenomena of dissociation might not take place to a certain extent; but what he did say was that neither the formation of nitric acid nor the formation of ammonia nor any of the phenomena connected with dissociation could account for the facts mentioned. He would only refer to two of those facts, namely, that notwithstanding the enormous loss of heat through the walls of the cylinder of a gas engine, amounting to 50 per cent. of the total amount of heat put into the cylinder, the curve of the indicator diagram still kept up the theoretical adiabatic line which it

should follow, supposing the whole of the gas were burned at the beginning of the stroke, and the walls of the cylinder were non-conducting. That was a startling fact which had to be dealt with in one way or another, but the interpretation of the fact seemed to him to be very simple, and even in the paper there were materials for arriving at a conclusion upon it. The author had stated that a mixture of gas and air took a certain time to ignite, that if ignition was set up at one point it took a certain time before it was communicated to another. There was also the further fact that at the rate of communication of the ignition from one point of the dilute mixture to another varied directly with the amount of dilution of the mixture. Supposing for instance there was a mixture of gas and air in the right proportions for explosion, the ignition would take place at a certain speed; if more air was put in, the rate would be less; the greater the quantity, the less the rate at which the ignition traveled. That simple fact he thought

sufficient to account for all the phenomena. The diagram which the author had given (Fig. 9) seemed to him, taken in conjunction with the fact to which he had referred, to support the theory which had been put forward by Mr. Otto and by the scientific world in general. In the Otto gas engine the charge varied from a charge which was an explosive mixture at the point of ignition to a charge which was merely an inert fluid near the piston. When ignition took place, there was an explosion close to the point of ignition that was gradually communicated throughout the mass of the cylinder. As the ignition got further away from the primary point of ignition the rate of transmission became slower, and if the engine were not worked too fast the ignition should gradually catch up the piston during its travel, all the combustible gas being thus consumed. When the engine was worked properly the rate of ignition and the speed of the engine ought to be so timed that the whole of the gaseous contents

of the cylinder should have been burned out and have done their work some little time before the exhaust took place, so that their full effect could be seen in the working of the engine. This was the theory of the Otto engine. What was the theory which the author had put forward? He had stated that when gases combined a high temperature was set up; that a high temperature prevented combination of the gases beyond a certain point; and therefore, at the moment of ignition, there existed in the cylinder a body of gases heated to a temperature beyond the point of dissociation. A part of those gases being in a state of combination, and having therefore given out a heat which was doing the work of pushing the piston; a part of the gases, not being in a state of combination, being ready to combine as soon as the temperature was lowered to such a point that they could combine and give out work. Looking at that theory, it seemed as if the point involved was a mere question of words, so far as regarded any question

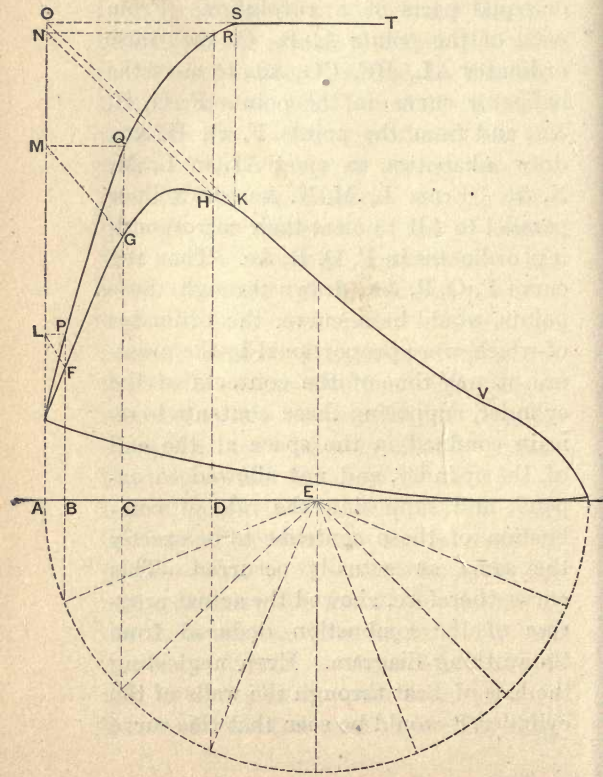
of infringement. In either case, what had to be dealt with was this. The adiabatic line represented the line which was traced out upon the indicator diagram when no heat escaped through the walls of the cylinder, and when the whole heat which the gases lost was converted into work done by the piston; so that, taking an indicator diagram, and finding the work done as represented by the area included by the curve, the ordinates and the atmospheric line, this work ought to be equal to the quantity of heat, represented in foot-lbs., which had been given out by the gas, as shown by the difference of temperatures and specific heat of the gas. Of course, when heat was escaping through the cylinder, and when the adiabatic line was still kept up to, a considerable amount of energy must be developed somewhere, in order to make up for the energy which went through the walls of the cylinder. The only source of energy in the gas engine was the union of combustible gases and oxygen, and it followed that that con-

stant supply of energy must come from the combustion of the gases within the cylinder. It was therefore a mere question of words, because, whether the energy was developed by the combustion of the gases which took place through the lowering of the temperature below the point of dissociation, or whether that energy was given out through the combustion of the gases which took place from the communication through the mass of an ignition which traveled slowly through it, in either case it was a gradual combustion. It was therefore a mere question of theory, and he did not see in what way it could affect the question of infringement. If Messrs. Crossley and Mr. Otto had overlooked the theory of dissociation, and had attributed the gradual combustion to something which they ought not to have attributed it to, he did not see how it could affect their position. The real point of difference, however, in a scientific point of view, between the author and himself was this. The author as-

sumed that the ignition was quickly transmitted through the cylinder, and took place almost at once near the beginning of the stroke, and that the ultimate combustion was due to dissociation; whereas Mr. Bousfield thought with Mr. Otto and many others that the cause of the supply of energy was the gradual communication of ignition through the contents of the cylinder. The author assumed gratuitously that when the point of maximum pressure was reached, that point marked the communication of ignition throughout the whole of the cylinder. That there was absolutely no ground for that assumption could be very readily shown. Neglecting for the moment the loss of heat through the walls of the cylinder, the curve representing the increase of pressure due to the combustion of the gas, supposing the gases to combine at the same rate as they actually did, but not to be allowed to expand by the motion of the piston, could be ascertained thus:—Divide the atmospheric line (Fig.

14) into spaces AB, BC, CD, DE, &c., representing equal small spaces of time or equal parts of a revolution. From each of the points A, B, C, &c., raise ordinates AL, BF, CG, &c., to meet the indicator curve in the points F, G, H, &c., and from the points F, G, H, &c., draw adiabatics to meet AL in L, M, N, &c. From L, M, N, &c., draw lines parallel to AB to meet their corresponding ordinates in P, Q, R, &c. Then the curve P, Q, R, &c., drawn through these points, would be a curve, the ordinates of which were proportional to the pressure at any time of the contents of the cylinder, supposing these contents to remain confined in the space at the end of the cylinder, and not allowed to expand, and supposing the rate of combustion of these contents to be exactly the same as actually occurred. This curve, therefore, showed the actual progress of the combustion deduced from the working diagram. Even neglecting the loss of heat through the walls of the cylinder, it would be seen that this curve

Fig. 14.

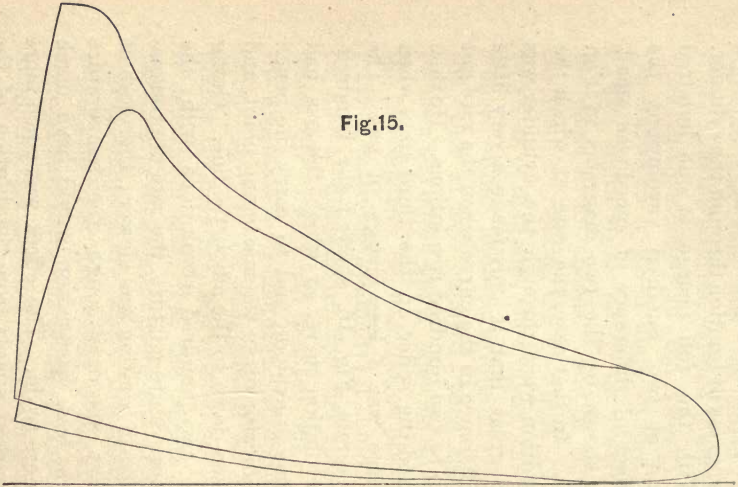


ascended to a point past the point of maximum pressure, viz., till the point K, at the commencement of the part KV (which was supposed to be exactly adiabatic) was reached. From the point S this curve became in the actual diagram a straight line parallel to AB. If, however, the theoretical diagram, allowing for loss by conduction, were taken, the curve PQRS would ascend throughout the stroke. Hence the maximum point on the diagram was simply the point where the increase of pressure due to combustion was balanced by the decrease of pressure due to the forward motion of the piston, and there was no reason for saying that this maximum point corresponded to complete ignition. He had had an opportunity of taking diagrams from the Otto gas engine, which Professor Ayrton had at the City Guilds Technical School, Cowper Street. The engine was designed for the electric light, and the cam, controlled by the governor, was made in a series of steps. He therefore had the governor taken

off, and the cam and the roller on which it acted so arranged that it should work independently of the velocity of the engine on a given step, so that the charge might be, as nearly as possible, the same at all speeds. And he varied the load by braking the fly-wheel. The two sets of diagrams were taken, one at a speed of one hundred revolutions, and the other at two hundred; thus might be seen the effect which must be due to the phenomenon he had spoken of—the ignition traveling gradually; it could not be due to dissociation, for the reason which Mr. Imray had pointed out. In the diagrams the phenomena of dissociation ought to be exaggerated at the higher temperature, but instead of that, it would be seen that the effects attributed to dissociation were less at the higher temperature where dissociation should be most active, and greatest at temperature below the point of dissociation; he therefore did not see why the results should be attributed to the phenomena of dissociation, when they could

be perfectly explained by the rate of progress of ignition through the cylinder. With the full charge at one hundred and at two hundred revolutions the effect of difference of speed was small, as shown by the two diagrams in Fig. 15. In that case, the rate at which the ignition went through the cylinder was so great that it only made a very little difference in the curve when the rate got up to two hundred revolutions. He then fixed the roller on the third step, when there was a less charge of gas. The diagram, Fig. 16, showed the hundred-revolution curve, in which the gas had time to explode, and to carry the pencil indicator up to the maximum point, and then down to the adiabetic line. Going to two hundred revolutions with the more dilute mixture, the rate of propagation of ignition was slower; therefore at that speed, although the temperature was less, dissociation would have much more to do. The effect was much more marked, simply from the dilution of the mixture; there was therefore a less rate

Fig.15.



4th step. 100 to 200 revolutions.

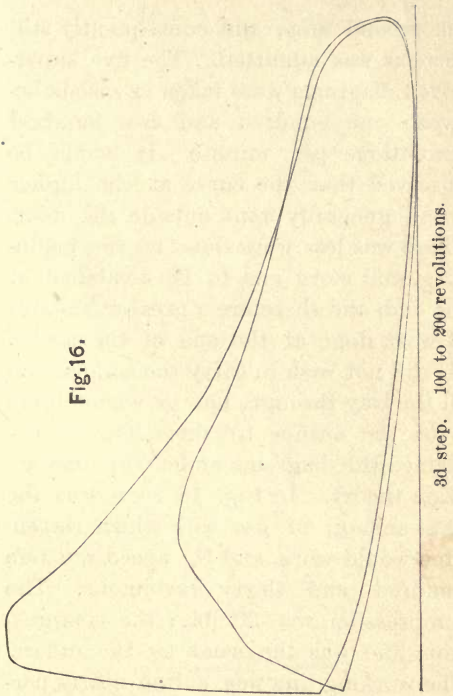


Fig.16.

of propagation of ignition, and the curve took the form shown in the diagram. Fig. 17 showed the same effects on the diagram when the curve roller was on

the second step, and consequently still less gas was admitted. The five superposed diagrams were taken at speeds between one hundred and two hundred revolutions per minute. It would be observed that the curve at the higher speed generally went outside the door. There was less work done at the beginning, and more gas to be combined at the end, and therefore a greater amount of work done at the end of the stroke. He did not wish to carry the comparison all the way through, but he would leave it to the author to show how he explained the diagrams under the dissociation theory. In Fig. 18 there was the least amount of gas with which the engine would work, and the speed was one hundred and thirty revoluions. The compression was 30 lbs.; the compression line was the same as the others. The working line was a line nearly parallel with the atmospheric line, but slightly rising, and at the end the ignition was not finished, indeed, in this case, if a light was applied to the ex-

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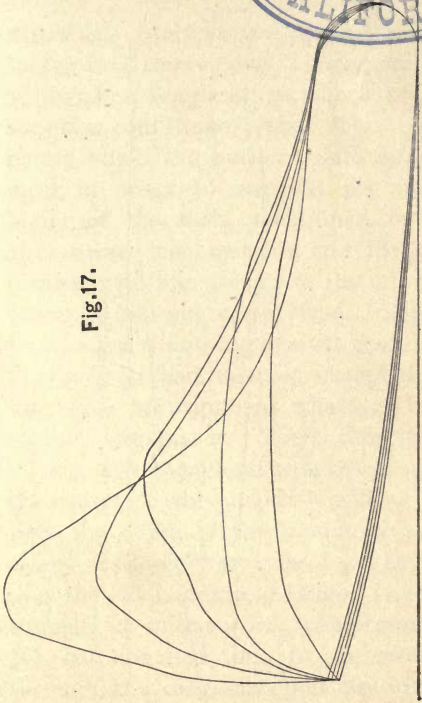


Fig.17.

haust the contents would explode. According to the author's theory, that maximum point near the end of the stroke in the last diagram was a point

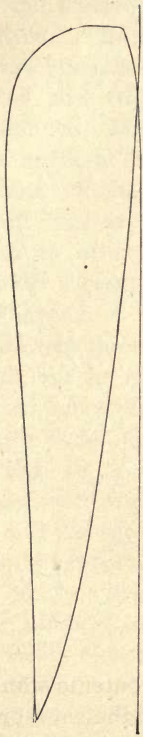


Fig.18.

where the ignition was complete, and therefore all the gas should have combined at that low temperature where no dissociation could take place. Those were points which the author would have to meet in order to support his theory. Many of the facts mentioned by the author were incontestable, and his chief dispute with him was as to the interpretation he had put upon them. The author had said nothing against the theory to which he had referred except that it was new, no argument whatever being advanced against it. The author stated, "From the considerations advanced in the course of this paper, it will be seen that the cause of the comparative efficiency of the modern type of gas engines over the old Lenoir and Hugon is to be summed up in one word, 'compression.'" He had not had time to go carefully through the diagrams; but he did not think that they were fair comparisons, and he thought that other elements ought to have been taken into account. The author had given the old Lenoir,

and had stated that the temperature was the same, that the mixture of gas was the same, and that the great advantage over the Lenoir was compression. Mr. Bousfield might be permitted to point out that, in the Lenoir engine, the adiabatic line was much above the actual line. It would be fairer to substitute the word "dilution" for "compression," so that the sentence would read: "The cause of the comparative efficiency of the modern type of gas engines over the old Lenoir and Hugon is to be summed in one word, 'dilution.'" The fact, however, was that it could not be summed up in one word; the two should be taken together, compression and dilution. The author further stated: "The proportion of gas to air is the same in the modern gas engine as was formerly used in the Lenoir." He did not think so. He believed that the Lenoir worked up to 13 to 1, and could not get further. He did not know what proportion Otto used, but it was considerably more than that. It was also stated that the time

taken to ignite the mixture was the same; but that was a gratuitous assumption. The author said: "The cause of the sustained pressure shown by the diagrams is not slow inflammation (or slow combustion as it has been called), but the dissociation of the products of combustion, and their gradual combination as the temperature falls, and combination becomes possible. This takes place in any gas engine, whether using a dilute mixture or not, whether using pressure before ignition or not, and indeed it takes place to a greater extent in a strong explosive mixture than in a weak one." Dissociation took place far more at high temperatures than at low; and if the author's application of the theory were correct the phenomena of dissociation ought to play a much greater part at high than at low temperatures. He had pointed out that this was not so in the diagrams, and that it was not so with Lenoir's explosive engines where the curve fell far below the adiabatic line. The paper contained other matters which he had not time to

dwell upon; but he thought he had said enough to challenge the author to show how he got rid of the old theory, and explained the facts to which Mr. Bousfield had referred.

Dr. JOHN HOPKINSON said a very interesting question had been discussed by Professor Rücker and Mr. Bousfield, to which he desired to refer. The author maintained that the ignition of the mixture of gases had extended throughout the whole space at a time approximately represented by the point of maximum pressure. Others, on the contrary, maintained that the ignition had not extended through that space by that time, but that it took a time lasting into the descending part of the indicator diagram before the disturbance had extended throughout the whole of that space. The author attributed the maintenance of the temperature during the latter part of the curve, and its approximation to an adiabatic curve, to the gradual combination of the gas through the mass, that combination not occurring com-

pletely in the first instance owing to the temperature being so high that a certain measure of dissociation occurred, or at all events so high that comptete combination could not occur. He thought that the question might be submitted to a crucial test. Suppose the opponents of the author were right, if a given mixture of air and gases were exploded in a gas engine revolving at a low rate of speed or in an entirely closed space, it would be expected that the maximum pressure would approximate to that calculated from the heat due to the combustion of the gas present and the temperature resulting therefrom. If the gine were running slowly, or if the explosion were made in a completely confined space, the pressure would be expected to rise to a point very greatly in excess of that observed in the gas engine running at its normal speed. Whether that were so he did not know. The experiment might be objected to on the ground that when the engine was running slowly there was a great loss of

heat through the walls of the cylinder. That would give rise to a second crucial experiment. If the author was right the maximum pressure in large and small engines would be about the same; if those who differed from him were right, in a large engine the maximum pressure would probably be greatly in excess of that in a small engine, there being less loss of heat through the walls of the cylinder. What the answer might be he did not know, but it appeared to him that there were there the elements of settling the question. The author divided gas engines into three classes, and had made a comparison of their theoretical efficiency. In the second the mixtures were admitted into the cylinder, and, without increase of pressure, the heat produced was devoted to increase of volume. In the third the mixtures were introduced into the cylinder, and then burned with an increase of pressure without immediate increase of volume; and in those two cases he took, for the purpose of comparison, different maximum pressures.

In the second type he took a pressure of 76 lbs., and in the third over 200 lbs. *Prima facie* it would seem natural, in order to make a fair comparison, that the same maximum pressure should be taken in the two cases. Probably the author had a good reason to justify his making a comparison on that basis, and, perhaps, in his reply he would point it out. He agreed with those who had so often spoken on the subject of the gas engine that in that engine lay the future of the production of power from heat of combustion. It was quite in its infancy, and it had already beaten the best steam engines in economy of fuel, for the obvious reason that it was practicable to use with it much higher temperatures. The steam engine tolerably approximated to the theoretical efficiency that might be expected from it, having regard to the temperatures between which it was practicable to work it. That was not the case with the gas engine, there being still a very large margin for practical improvement. Having regard to the very short

time during which gas engines had been used, he thought that practical improvements would take place, and that, when such difficulties as that of starting a large engine as conveniently as steam engines could be started had been overcome, the gas engine would supersede the steam engine.

Mr. E. F. Bamber wished the author had commenced his paper with that portion which treated of the analysis of the gas, and had given the mechanical equivalent of a unit of the same both in the pure and diluted state. If the explanation had then followed, that the mechanical equivalent of the latent heat of expansion per unit of the gaseous mixture per degree of temperature was nearly the same as for atmospheric air, the reason why the gas engine might be considered in theory as an air engine would have been clearer, namely, that the adiabatic curve, or curve of no transmission of heat, was nearly the same for both. The author commenced by an attack upon the steam engine. Much heat was required

in evaporating water whose specific heat was high, and hence the efficiency of the steam engine was low, and something better was needed; whereas it was clearly proved by Rankine, a quarter of a century ago, that the maximum efficiency of a theoretically perfect heat engine, working between given limits of temperature, was equal to the ratio of the range of temperature to the higher absolute limit of temperature, and quite independent of the fluid employed. Raising the temperature entirely by compression or using regenerators were the two means by which the actual efficiency might be made to approach the maximum limit. The author believed in compression, but his method of defence of it and his illustrations of its advantages did not appear to be quite correct. He took three types of engine: the first and third were explosive gas engines; the second was worked at constant pressure, and these he treated as air engines. The first and second were worked between the same limits of temperature, but in the second

compression was employed. What the author wished to prove by the theoretical diagrams of these types was that the constant-pressure engine using compression was more theoretically perfect than an explosive engine using none, whilst an explosive engine using compression was the best of the three. But he had shown by type No. 2, that by the use of compression an efficiency could be attained higher than the maximum efficiency of a perfect heat engine, which seemed to require some explanation. The maximum was equal to $\frac{\tau_1 - \tau_2}{\tau_1}$ in absolute degrees of temperature, and was for 1,537° Centigrade and 1,089° Centigrade equal to 0.247 for both types; whereas the author made it 0.21 for the first and 0.36 for the second. The author allowed that type No. 1 would be improved by further expansion, but that that would require a vacuum pump and condenser; yet surely it made no difference, so long as they both consumed the same quantity of heat, whether a com-

pression pump was used at the beginning or a vacuum pump at the end of the stroke, whilst indeed there might be theoretical reasons in favor of the latter. Types 1 and 3 were respectively worked without and with compression; they were both explosive engines, and the efficiency of the latter was made double that of the former, but the latter was made to discharge at 648° Centigrade, and the former at 1,089° Centigrade. If these figures had been reversed, so would have been the efficiencies. Had the author explained that there was a certain maximum efficiency for heat engines, and that by means of compression a larger percentage of that maximum could be attained than without it, there would have been no reason for objection; but that was a very different thing from trying to show that it was possible to obtain more than the maximum efficiency of a theoretically perfect heat engine.

The real value of the gas engine was, that it contained the furnace and engine in one; thus the necessary heat lost in

the furnace to make a draught, and the unnecessary loss of heat by radiation from a large steam boiler were both avoided in the gas engine, and, finally, the gas engine could be used safely at a maximum limit of temperature, which could not be employed in the steam engine. There was no doubt a great future for this class of motor.

Sir William Thomson said that he had recently seen a very interesting experiment made by the author with a gas engine at Glasgow, which he thought had a most important bearing on the mode of action of the gas in the cylinder. The experiment was made in the presence of his brother Professor James Thomson and Professors Jack and Ferguson (of Mathematics and Chemistry in the University of Glasgow), who were all much interested in the inquiry. The object was to test the nature of the mixture in close proximity to the piston, so as to be able to form some idea as to whether or not the explosion took place through the whole space; to be judged by finding

whether, right up to contact with the piston, gas and air were present in proportions suitable for combustion. He need not enter into details as to the way in which the experiment was made, but he might say, in a general way, that while the piston was being pressed in to condense the mixture at a definite point of the stroke, communication was made with the cylinder. The small experimental cylinder and piston were placed in proper position, in communication with an aperture bored for the purpose in the main cylinder. The author of the paper would be able to explain the details better than Sir William Thomson could. It was sufficient to say that by an automatic arrangement, worked mechanically from the cross-head, the communication was made exactly at one definite point of the stroke, and the experimental piston was pressed up in the cylinder so as to let it fill. At any time afterwards the stop-cock could be opened by hand, and the nature of the contents tested. In every case the contents were found to be ex-

plosive—an explosive mixture of gas and air—proving that up to the very point, which he understood was within about an inch from the piston, coal gas was present in suitable proportions for producing an explosion. There was one other matter to which he wished to refer, which had been noticed in the discussion. There appeared to be some difference of opinion upon it, but to his mind it scarcely appeared open to doubt—that the diagram, which showed an exceedingly sudden rise and a gradual fall, proved that combustion was practically complete at a point corresponding to the summit of the curve. Literally and precisely the instant of the maximum of the curve was that at which the rate of loss of pressure by expansion, the much smaller rate of loss of pressure by loss of heat carried by convection of the fluid to the solid boundary and out by conduction through the metal, were exactly counterbalanced by the rate of combustion still going on. It seemed certain that the rate of loss by the two causes he had indicated was ex-

ceedingly small in comparison with the rate of rise by the initial progress of the explosion; therefore, practically speaking, the maximum of the curve indicated truly the instant when the combustion was as complete as dissociation at the highest tempearature attained allowed it to be.

Mr. D. Clerk, in reply upon the discussion, said that two of the speakers seemed to think that the question at issue was one of infringement of patent, but he desired to arrive at the truth, apart from mere questions of personal interest. The question of infringement was to him one of complete indifference.

The question he was anxious about was the purely scientific one. Was his theory of the action of the gas engine the true one, or was it Mr. Otto's? This matter might appear to some persons a small one, but he considered it of vital interest, being convinced that not many years hence the gas engine would have a science of its own, and scientific names connected with it as much honored as any ever linked with the steam engine. Dr. Sie-

mens had fully corroborated his view of dissociation, and in the effect it had on the gas engine diagram, in preventing the more rapid fall, which must otherwise occur; but he did not agree with him in the necessity for further research on dissociation, believing that St. Claire Deville's work was sufficient. Dr. Siemens would observe that St. Claire Deville's researches were referred to in the paper; but what he asked for had never to his knowledge been published, that was a complete curve of the dissociation of water and carbonic acid. St. Claire Deville's results were more of a qualitative than of a quantitative nature. He feared that the method used was not capable of the necessary accuracy.

He thoroughly believed that the engine for the very large powers to be constructed in future must be of one type 2, with hot chamber or cylinder, and regenerative contrivance in some form; indeed, about two years ago he constructed and experimented with such an engine, and he was continuing his experiments.

The mechanical difficulties were much greater than in the cold cylinder, type 3. It must be remembered that the cold cylinder gas engine was the engine of the present, and it was most satisfactory that even with the small sizes so high a duty should be obtained. It proved that when larger engines were made a much higher duty might be expected. The theory of the cold cylinder engine did not allow of the application of any regenerative contrivance, and consequently arrangements must be made to get the greatest possible fall of temperature due to work done. A very interesting account had been given by Professor Rücker of his view of the problem, and the necessity of correcting the calculations of previous observers in the light of present knowledge of the laws of combustion had been demonstrated. It was satisfactory that Professor Rücker so thoroughly agreed with him on the necessity for considering dissociation in any theory of the gas engine, and had independently arrived at similar conclusions. The experiments of Messrs.

Mallard and Le Chatelier corroborated those of Professor Bunsen in this, that at the high temperature of combustion, a large amount of heat was rendered latent. So striking a fact could hardly have escaped the notice of many other experimenters who might not have published their results. He had noticed it about five years ago, while making experiments on the maximum pressure obtainable from a pure explosive mixture of gas and air. A cylinder 9 inches in diameter and 9 inches long, was filled with a mixture of gas and air in the proportions for maximum explosive effect, and ignited the mixture by means of a hollow stop-cock, after Barnett's style of igniting arrangement. With the temperature of the mixture before ignition at 12° Centigrade, the highest pressure attained was 97 lbs. per square inch above the atmosphere. The pressure was measured by a loaded valve of known area, as in Bunsen's experiments. The absolute pressure attained was only about 7½ atmospheres; if complete combination had taken place,

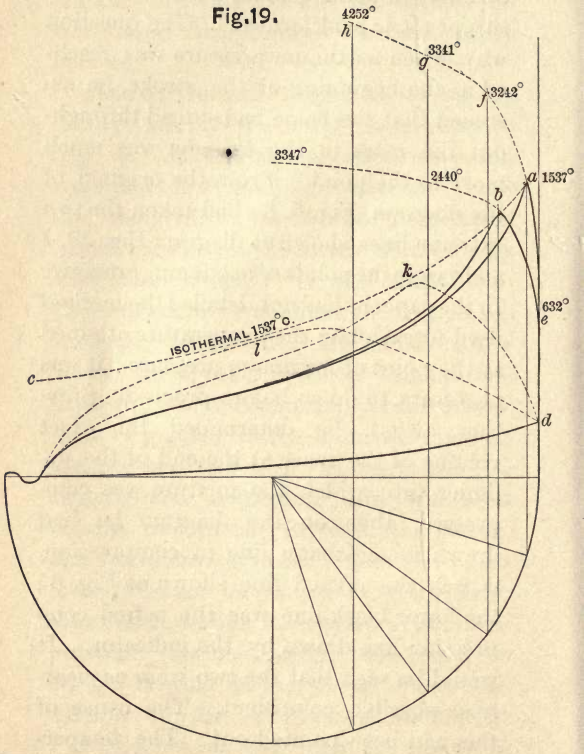
and no heat kept back by dissociation or absorbed by change in specific heat, then the pressure should have been at the lowest estimate, 11 atmospheres. He concluded that Professor Bunsen's explanation of this fact was a true one. The effect was equally visible in the large cylinder used by him and in the small tube used by Professor Bunsen. These experiments, and the recent experiments of Messrs. Mallard and Le Chatelier, make it certain that in a uniformly ignited gaseous mixture the temperature was limited, and the apparent loss of heat was very slow, and that this effect was due to dissociation, either complete or incipient. Such a mixture in expanding during work would give rise to all the phenomena described in the paper. He was pleased that his conclusions on the relation between rate of inflammation at constant pressure and constant volume had been experimentally proved by these gentlemen. He had been challenged by Mr. Imray to controvert his statement on the history of the introduction of the gas

engine. This he did not do, because he considered Mr. Imray's account fairly correct.

The only remark of Mr. Imray on his theory was: "He would only refer to Fig. 9. If the theory of dissociation were true, it would follow that the lower the temperature the more dissociation would take place, which was undoubtedly altogether wrong." It was difficult to understand this statement, it was so exceedingly irrelevant. He could hardly believe the speaker had ever studied the pressure, volume, and temperature relations of gases. On the indicated diagram low pressure had been mistaken for low temperature, neglecting the increased volume due to the travel of the piston. Mr. Imray had supposed that the maximum pressure on line *d* (Fig. 9), being lower than on line *a*, therefore the temperature was also lower. He failed to see the bearing on the theory under discussion of Mr. Bousfield's statement: "He did not say that when the explosion took place, there might not be a certain

quantity of ammonia and a certain quantity of nitric acid formed." The question why, when maximum pressure was reached at the beginning of the stroke, he assumed that the flame had spread throughout the mass in the cylinder was much more to the point. From the original of the diagram, Fig. 6, he had taken the two extreme lines shown at diagram Fig. 19, *a* and *b* were the points of maximum pressure. In the paper he had not detailed the method used to calculate the temperature attained at the point of maximum pressure; it was necessary to do so before proceeding further. First, he determined the exact volume of the space at the end of the cylinder into which the mixture was compressed, then on the diagram he had drawn the adiabatic line of compression, it was the dotted line shown at Fig. 6; the lower black line was the actual compression line drawn by the indicator. It would be seen that the two were as nearly as possible coincident. The cause of this had been pointed out. The temperature at the point *c* was known to be

Fig.19.



Engine speed 150 revolutions per minute.
One division of circle=one-fiftieth part of a second at above speed.

150°.5 Centigrade, and the pressure 41 lbs. above atmosphere, and assuming the volume to remain constant, the temperature at *a* was calculated from the pressure 220 lbs. above atmosphere.

Let P = pressure before ignition, and P′ pressure after ignition, T = temperature before ignition, and T′ temperature after ignition, then—

$$T' = \frac{P' T}{P}$$

both pressures and temperature absolute. In diagram Fig. 1 it was shown that the temperature of compression, corresponding to 40 lbs. above the atmosphere, was 150°.5 Centigrade, and from these figures the temperature 1,537° was obtained. This was the minimum possible temperature, as would be observed from certain considerations developed at p. 21. Whether the flame had spread throughout the mass of the mixture or not, this was the average temperature. From *a*, Fig. 19, was drawn an isothermal line, *a b c*, dotted ; at the point *a* the temperature had commenced to fall, up to that

point it had been rising at a very rapid rate. The semicircle drawn below the atmospheric line showed the path of the crank-pin, and each division represented in time one-fiftieth of a second; the engine was running at one hundred and fifty revolutions per minute when the diagram was taken. Comparing the condition of the gaseous mixture in one-fiftieth of a second before maximum pressure, and one-fiftieth of a second after maximum pressure, in the first one-fiftieth of a second the average temperature had increased 905° Centigrade, while in the second hundredth it had diminished about 189° Centigrade. Within a limit of one twenty-fifth of a second there was a point where the increase of temperature ceased, and where a fall of temperature began. What did this mean? Why did the increase of temperature cease in so sudden a manner and a fall of temperature set in?

From the point *d* to *a* the temperature had been increasing, this increase being due to the progress of the flame; at the

point *a* the increase ceased, and a fall set in. Take the point *e*, then the average temperature was 632° Centigrade; from *e* to *a* the time taken one-fiftieth of a second, and the temperature rose to 1,537° Centigrade; in that time it had increased by 905°; suppose the same rate of increase to continue for another one-fiftieth of a second, the pressure would rise to the point *f*, and the temperature would be 2,442° Centigrade, the points *g* and *h* showed the effect of further increase. But the increase had abruptly ceased at the point *a*; from *a* to *f* the volume had changed so slightly that the rate of cooling could not have increased appreciably. The amount of work done in that movement was also relatively insignificant, and yet from some cause the increase of temperature going on with such rapidity, 905° in one-fiftieth of a second, had not only diminished, but an opposite effect had set in. It could not be supposed for a moment that the progress of the flame had been abruptly stopped by any cause other than com-

pleted inflammation of the whole mass. The flame which in one instant of time had been flashing through the explosion mixture had reached the enclosing walls, it had uniformly heated the whole combustible mass, and in the next instant the temperature began to fall; the law of cooling took effect. The very rapid rate of rise, and the abrupt change from rapid rise to slow fall of temperature, at a given point, showed that at that point completed inflammation had been attained. The cooling which was so slow as to be unable to put an appreciable check on the rate of rise up to the point of maximum temperature, could not be supposed to suddenly increase to such an enormous extent as to completely absorb and overpower at that instant the effect of continual spread of flame. There could be no doubt that, as Sir Thomson had pointed out, on diagram Fig. 6, the maximum of the curve indicated truly the instant when the combustion was as complete as dissociation allowed it to be. It was certain that at this point of the diagram the flame had

spread completely through the whole volume of inflammable mixture, and that in whatever way the sustaining of the pressure to nearly the adiabatic line was to be explained, it could not be accounted for on the hypothesis of a continued s read of flame.

A little consideration of the conditions of the indicated diagram would show that the slower the rate of inflammation, relatively to the movement of the piston, the less distinct would the point of maximum pressure become, and the more rounded would the apex of the diagram appear. Nevertheless the point of completed inflammation was easily determined from the point of maximum temperature, when near the end of the stroke this point might not be the point of maximum pressure. He had been careful to make this distinction, and had said, with reference to slow inflammation, p. 25: "This supposed phenomena has been erroneously called slow combustion; if it has any existence it should be called slow inflammation. It has a real existence in

the Otto engine only when it is working badly; but even then maximum temperature is attained, and very distinctly marks the point of completed inflammation." On diagram Fig. 19 was shown the effect of increasing the speed of the engine while preserving a constant rate of inflammation. If the speed were increased from one hundred and fifty revolutions per minute three times, or to four hundred and fifty revolutions per minute, it would be found that the point *a* would be moved forward to *k* and *b* to *l*. In both cases the temperature attained would be nearly 1,537° Centigrade, a slight fall would be observed due to increased cooling surface and to a part of the work being done before maximum temperature was attained. But in all cases the maximum temperature marked the point of completed inflammation and the temperature began to fall so soon as it was attained. For ignitions attaining their maximum very late in the stroke, maximum pressure need not coincide with maximum temperatures; but a reference

to the isothermal line showed the point of highest temperature. Using an inflammable mixture of constant composition, and varying the speed of the engine, it was always found that ignitions attained maximum temperature later and later in the stroke always came very near the isothermal line drawn from the point of highest pressure at the beginning of the stroke. The lines never ran over this isothermal. This meant that, whether inflammation was completed early or late in the stroke, nearly the same maximum temperature was attained. It followed from the relations between isothermal and adiabatic lines, that the lines drawn by the indicator from late ignitions always crossed those from early ignitions. This was shown by the diagrams taken from an Otto engine by Mr. Bousfield, for which he must thank that gentleman. In these diagrams, however, it was evident that the mixture used had not been of constant composition at all speeds. This would be evident by examining Fig. 15. When the speed had been changed from

one hundred revolutions per minute in the larger diagram to two hundred in the smaller, the increased speed of the engine had caused it to take in a smaller weight of gaseous mixture, as was shown by the compression line leaving the atmospheric line later, and that the pressure on completion of the in stroke only rose to 22 lbs. per square inch instead of 30 lbs., as in the other. If the mixture had been the same the point of maximum pressure would have crossed in the first diagram at this point, and the pressure line would have run into the first lower down, as was shown in his diagram at *b*, Fig. 19. In the Otto engine the hot exhaust remaining in the space when each cycle was completed still further complicated the comparison between different speeds. At the higher speeds the walls of the cylinder had less time to cool the exhaust, and consequently the average temperature of the mixture before compression must be greater at high speeds. In his own gas engine this complication had no existence, because the whole charge was replaced at

every stroke. In Mr. Bousefield's diagram, Fig. 16, the same change of mixture was evident, but here the change of speed of the engine was relatively greater, and consequently the lower diagram crossed the upper one somewhat earlier. In Fig. 17 this was more and more evident; still no two of the compression lines coincided, showing the proportion of exhaust to inflammable mixture to be continually increasing, and the maximum temperature attainable by the ignition consequently becoming less and less. Even in diagram, Fig. 18, maximum temperature was attained, and could easily be discovered by calculating the average temperature at each point along the line of increasing volume. Mr. Bousfield stated that a light applied to the exhaust of an engine, giving diagram, Fig. 16, caused explosion, and from that inferred that combustion was not completed at the end of the stroke. He would find that when this happened the engine was missing ignition altogether and discharging the unburned contents into the ex-

haust. He might observe that the horizontal line in that diagram did not mean constant temperature, but indicated constantly increasing temperature. Mr. Bousfield has evidently fallen into the same error as Mr. Imray, and confounded low pressure with low temperature without considering the change of volume. It was a characteristic of the inflammation of a gaseous mixture in mass, that so long as inflammation continued to spread, so long did the average temperature increase. Dissociation did not begin to sustain temperature until the temperature fell. In the construction of the theoretical diagram Mr. Bousfield had fallen into error. He drew from the points F G H, Fig. 14, to A L produced, lines which he described as adiabatics, and then said that the curve drawn through P Q R "represented the pressure at any time in the contents of the cylinder, supposing these contents remain confined in the space at the end of the cylinder, and not allowed to expand." Now the lines F G H should not be

adiabatics but isothermals, as Mr. Bousfield's object in constructing the diagram was to get the time taken in a closed space to attain the temperature existing in the engine at the points F G H. The points L M N should show the pressure at constant volume at these temperatures. If Mr. Bousfield calculated the temperature from an actual diagram, he would find that maximum temperature coincided with maximum pressure when at the beginning of the stroke. He thought from his remaining criticisms that Mr. Bousfield had not understood the nature of the proof advanced in the paper, and that when he had studied the subject and appreciated the nature of the considerations advanced, he would admit the truth of the theory set forth in the paper.

It had been asked by Dr. Hopkinson whether the pressure rose higher when an engine was running slowly than when it was running fast? Whether the pressure attained on exploding a gaseous mixture in a closed space and in an engine

was the same? Given the same proportion of gas to air and the same temperature and pressure of mixture before ignition, then the pressure attained after ignition was the same in all stages where the maximum pressure was attained at the beginning of the stroke; it was the same whether in a closed space or in an engine. But the ignition must be rapid enough at the higher rate of speed to give maximum pressure at the beginning of the stroke. As he had already pointed out, if an engine was to run fast enough it might overrun the rate of inflammation, and the maximum temperature would not be attained till towards the end of the stroke. If an engine was run at two hundred revolutions per minute and maximum pressure was attained at the beginning of the stroke, then however slowly that engine ran using the same mixture, the maximum pressure would always be the same, it would not increase. Dr. Hopkinson then asked, Was the maximum pressure the same in large and in small engines? When using a similar

mixture, the same pressure and temperature before ignition, it was the same. In small engines the temperature fell more rapidly than in large ones because of the greater proportion of cooling surface to volume of gases, but the maximum pressure attained was nevertheless the same because of the rapid rate of ignition. The results obtained in the large cylinder to which he had alluded, and those obtained by Professor Bunsen in a small tube, each showing a limit to the rise of temperature which could not be referred to cooling, and each showing complete spread of flame, proved that the maximum pressure to be obtained from an explosive mixture was independent of the dimensions of the vessel used. Dr. Hopkinson had asked why, in comparing types 2 and 3 of engine, he used different maximum pressures; why in the second type he used 76 lbs. per square inch above the atmosphere, and in the third over 200 lbs. per square inch. His reason was this: the three types were taken under conditions which have been found in practice

to be the most favorable for each. He had compared the theory of these types of engine as nearly as possible under conditions used in practice. It was quite true that type 2 should be compared with type 3 under similar conditions of pressure from a purely theoretic standpoint; but the object of the paper had been to inquire into the cause of the greater efficiency of the third type as in use against the two first also in use. It would be seen that to attain a pressure of 200 lbs. per square inch in type 2 it was necessary to compress the mixture to that pressure before ignition, the temperature of compression being nearly 365° Centigrade. This involved considerable loss of heat in the reservoir, and increased the chances of leakage while compressing; in type 3 a pressure of 40 lbs. per square inch before ignition was all that was required to attain 200 lbs. after ignition. He believed that type 2 could work advantageously at a much higher pressure than 76 lbs. per square inch, but he questions whether it could do so at so high a pressure as 200

lbs. The advantage of type 3 in this respect was a comparatively low pressure before ignition. With careful workmanship doubtless it would be possible to use an engine of type 2, the theoretical efficiency of which would be quite as much as type 3, as given in the paper.

The description by Mr. F. H. Wenham of his work on hot-air engines was interesting, and his distinction of the cylinder itself as the heat generator or furnace was the essential one between gas and hot-air engines, and was indeed the great cause of success in these engines. Mr. H. Davey had objected to his comparison of the efficiency of gas and steam engines, and considered the basis of comparison of efficiency used by him as an unfair one. In comparing engines of the same system it was right, as Mr. Davey stated, to use as the standard the mechanical equivalent of the total available heat; but in engines of totally different nature the only basis of comparison was the number of heat-units given to the engine, and the number of these heat-units con-

verted into mechanical work. If one system was necessarily limited in range of temperature, as the steam engine was, then the inquiry must not be how near it approached perfection within that range, but how much heat could another system convert into work as compared with it. In comparing steam engines with steam engines Mr. Davey is perfectly right; in comparing with gas engines the general basis must be taken. He agreed that the speedy downfall of the steam engine was not to be anticipated; he only held that the gas engine was now in its infancy, that it contained greater possibilities than the steam engine, and that in the future it was certain to be in every way a great advance on the steam engine, and likely to supersede it.

The propriety of treating the gas engine as an air engine had been called in question, and he had been asked whether the specific heats of air and the gaseous mixture used were in any way comparable. The specific heat of air at constant volume was 0.169, and the specific heat of a

mixture of 1 volume of coal gas and 12 volumes of air could not exceed 0.200, so that for the purpose of approximate comparison their adiabatic curves might be considered as nearly identical. So little was known of the specific heat of gases at high temperature that Mr. Clerk considered it simply an affectation of accuracy to endeavor to make the comparison closer. He was aware that the efficiency of a heat engine was independent of the nature of the fluid employed, provided the temperatures between which the engines worked were the same—that was provided there was the same difference between source and refrigerator. But this was just where the steam engine failed. Given equal amounts of heat from the same source, in the steam engine the high temperatures could not be utilized, because, first, a certain quantity of heat had to be expended to change the physical state of the water; and as the steam produced was rejected as steam all the heat so expended was lost for the purpose of procuring high temperature.

With air, on the other hand, the same quantity of heat from the same source, a much higher temperature was attained, and consequently a greater range of temperature due to work performed. The use of steam necessitated a limited range of temperature, and the discharge of all the heat used in converting water from a liquid to a gas. It had been argued that in engine type 2 he had over-estimated the efficiency, and made it greater than was possible from a perfect heat engine working between the limits of temperature used. Mr. Bamber had fallen into error by mistaking the limits, and in this he was not alone. This type of engine presented very interesting peculiarities in theory, which, so far as he was aware, had hitherto been missed by writers on thermo-dynamics. Although 1,537° Centigrade was the maximum temperature, and 1,089° Centigrade the temperature of discharge with the exhaust, yet these temperatures were not the limits within which the engine was working; the refrigerator, which was at atmosphere

temperature 17° Centigrade, was being used to a certain extent without being apparent.

The diagram was not a simple one; the efficiency 0.36 was the result of the united action within two different limits. The diagram from 1,537° Centigrade to 1,089° Centigrade was the same both in types 1 and 2, and working between these limits the maximum possible efficiency was 0.247; but in type 1 this efficiency was not attained, because at 1,089° Centigrade the air had not the same density as before expansion, and some work had been expended in changing the volume to twice its original amount. If before heating the air had been compressed slightly, then heated to 1,537° and expanded to its original volume, and lowered in temperature due to work done to 1,089°, the duty would be 0.247. If in type 1 a condenser were used, and the temperature reduced to 17° Centigrade, the additional work obtained would raise its duty to 0.247, without this it remained at 0.21. In both types the efficiency between the

limits 1,537° Centigrade and 1,089° Centigrade was the same; but in type 2 a considerable amount of work was obtained in the earlier part of the diagram, a certain amount of work was done on increasing temperature from 217°.5 Centigrade to 1,537°, and a considerable proportion of heat could be converted into work on an increasing temperature, still conforming to the law $\frac{T_1 - T_2}{T_1}$ as the maximum possible between the limits.

In type 2, to a certain extent, the refrigerator at atmosphere temperature was made available in a portion of the action, and consequently a portion of work done on increasing temperature, while the latter half of the stroke was accomplished on falling temperature. This was the reason why a greater efficiency was got than the apparent limits would allow. Mr. Bamber then argued that it made no difference whether it was necessary to use an air pump or not, if only the same quantity of heat were consumed and the same theoretic efficiency obtained. In

practice it made all the difference; the great cause of failure with hot-air engines was not imperfect theory but very low available pressures combined with high maximum pressures. Nearly all the power indicated was used up in friction; in the earlier gas engines the average pressures were very low also. The advantages of compression were a high available pressure, small cooling surfaces, and small loss by friction. There the efficiencies depended on the range of source and refrigeration; but compression allowed all this to be attained under practical conditions. It was hardly necessary to explain that there was a certain maximum efficiency for heat engines. What he had shown in this paper was that a greater proportion of this was possible under working conditions with compression than without.

The parallel by Mr. Cowper between slow inflammation and imperfect admission of steam in a cylinder was very just, and illustrates the great loss of power and heat involved by imperfect mixing

of gas and air, or by failing to attain maximum pressure as soon after firing as practicable. It was only by a constant application of theory to practice, and a constant testing of results obtained by varying conditions, that he had been able to produce the diagram which Mr. Cowper approved. The amount of gas consumed by his 6-HP. engine was 22 cubic feet per 1 HP. per hour. Of course in cost this did not stand comparison with the coal used by a large modern steam engine; the steam engine had greatly the advantage; but compared with a small steam engine it was economical. When gas was manufactured expressly for gas engines it need cost but little more than the coal used to produce it, and as the gas need not be illuminating all the carbon might be converted into gas. The gas might be in fact a mixture of carbonic oxide and hydrogen.

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